

THE

NATURAL HISTORY

OF

ANIMALS, VEGETABLES,

MINERALS, &c.



1570/6080

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NATURAL HISTORY

OF

ANIMALS, VEGETABLES,

AND

MINERALS;

WITH

The THEORY Of the EARTH in general.

Translated from the FRENCH

Of COUNT de BUFFON.

Intendant of the Royal Gardens in France; Member of the French Academy, of the Academy of Sciences, and of the Royal Societies of London, Berlin, &c.

By W. KENRICK, L.L.D. and OTHERS.

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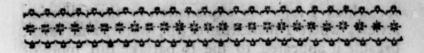
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NATURAL HISTORY

O F

MINERALS.

On LIGHT, HEAT, and FIRE, continued.

** HESE stones become hard by the long heat they have endured, and become at the fame time specifically heavier. From this circumstance I have thought it necessary to draw an induction, which proves, and even fully confirms, that heat, although in appearance always fugitive and never stable in the bodies which it penetrates, from which it feems constantly trying to quit, nevertheless, deposits in a very stable manner many parts which fixes there even in a greater quantity, the aqueous and other parts which it has driven off. But what appears contrary, or at least very difficult to be here reconciled is, that this fame calcareous stone which becomes specifically heavier by the action of a moderate heat a long time continued, becomes all at once near a half lighter when it is submitted to a fire great enough for its calcination, and that it, at the same time, loses all the hardness it had acquired by the action of heat, but even its natural hardness, that is to say, the adherence of its conflituting parts: a fingular effect, for

for which I refer the explanation to the following article, where I shall treat of air, water, and earth, because it appeared to me to partake still more of Nature in these three elements, than in that of fire.

But this is the place to speak of calcination generally received; it is with respect to fixed and incombuffible bodies what combustion is for volatile and inflammable: calcination, like combustion, has need of the affistance of air; it operates so much the quicker, as it is furnished with a greater quantity of air; without that the fiercest fire cannot calcine nor inflame any thing, except the matters which they contain in themselves, and which supply in proportion as they burn or calcine, all the air necessary towards the combustion or calcination of substances with which they are mixed. This necessity for the concurrence of air in calcination as in combustion, indicates, that there are more things common between them than has been suspected. The application of fire is the principal of both; that of air is the fecond cause, and almost as necessary as the first; but these two causes are equally combined, according as they act in more or less time, and with more or less power on different substances. To reason justly, we must recall the effects of calcination, and compare them between themselves and with those of combustion.

Combustion operates suddenly, and sometimes instantaneously; calcination is always slower, and sometimes so long as to be thought impossible: in proportion as matters are more incombustible, the calcination is the more flowly made: and when the constituent parts of a substance, such as gold, are not only incombustible, but appear so sixed as not to be volatilized, calcination produces no effect, however violent it may be. Calcination and combustion, therefore, must be considered as effects of

the fame class, whose two extremes are delineated to us by phospnorus, which is the most inflammable of all bodies, and by gold, which is the most fixed and least combustible. All substances comprised between these two extremes, will be more or less subjected to the effects of combustion and calcination, according as they approach more or less these two extremes; infomuch, that in the middle points there will be found substances which will endure an almost equal degree of combustion and calcination: from whence we may conclude, without being in danger of being deceived, that all calcination is always accompanied with a little combustion, and that likewise all combustion is accompanied with a little calcination. Cinders, and other refidue of the most combustible matters, do not they demonstrate, that fire has calcined all the parts it has not burned, and, confequently, a little calcination is found here with combustion? The little flame which raises from most matters that are calcined, does not it demonstrate likewise, that a slight combustion is made? Thus, we must not separate these two effects, if we would lay hold of the refults of the action of fire on the different substances to which it is applied.

But it will be faid, combustion destroys bodies, or at least always diminishes the volume or mass, by reason of the quantity of matter it consumes. Calcination often does the contrary, and increases the weight of a great number of matters. Must we from hence consider these two effects, whose results are so contrary, as effects of the same class? The objection appears founded, and deserves an answer, so much the more as this is the most difficult point of the question: nevertheless, I think I can answer it fully. Let us consider a matter in which we shall suppose one half to be fixed parts, and the other volatile or combustible. By the application of fire, all these volatile or combustible parts will be raised

up or burnt, and, confequently, separated from the whole mass: from hence this mass or quantity of matter will be found diminished one half, as we see it in calcareous stones which lose near half their weight in the fire. But if we continue to apply the fire for a very long time to this half quite composed of fixed parts, is it not easy to conceive, that all combustion, all volatilisation being ceased, this matter, instead of continuing to lose its mass, must, on the contrary, acquire some by the expence of the air, and the fire with which it is penetrated; and those which, like lead, do not lose any thing, but gains by the application of fire, are matters already calcined, and prepared by Nature to the degree, where combustion ceases, and, consequently, sufceptible to augment the weight from the first moment of the application. We have feen, that light extinguishes on the furface of all bodies which do not reflect; we have observed, that heat, by its long refidence, fixes partly in the matters which it penetrates: we know, that the air, almost as necesfary for calcination or combustion, and always so much the more necessary for calcination as matters have more fixity in the external parts of bodies, and becomes a constituent part: hence, is it not very natural to imagine, that this augmentation of weight proceeds only from the addition of the particles of light, heat, and air, which are at length fixed and united to one matter, against which they have made fo many efforts, without being able either to raife them up or burn them? This is so true, that when we afterwards present a combustible substance, with which they have much more analogy, or rather conformity of Nature, they will greedily lay hold of it, quit the fixed matter, to which they were only, as I may fay, attached through force; retake, confequently, their natural motion, their elafficity, their volatility, and all depart with the combustible matter, matter, to which they are just joined: from hence, metal, or calcinized matter, to which these volatile parts has been rendered, that which it had loft by combustion, re-takes its pristine form, and its weight is found diminished from the whole quantity of flery and airy particles which were fixed in it, and which had been just raised by this new combustion. this is performed by the fole law of affinities; and after what has been faid, there feems to me to be no more difficulty to conceive how the lime of a metal is reduced, than to understand how it is precipitated in diffolution: the cause is the same, and the effects are fimilar. A metal diffolved by an acid, will precipitate when to this acid another substance is offered with which it has more affinity than metal, the acid then quits it and falls to the bottom. So, likewise, this metal calcines; that is to fay, is loaded with parts of air, heat, and fire, which being fixed, keeps it under the form of a lime, will precipitate, or if you chuse, will be reduced when prefented to this fire and fixed air, from the combuftible matters with which they have more affinity than with the metal, which will re-take its first form as foon as it shall be disembarrassed from this superfluous air and fire, and that it will have re-taken, at the expence of the combustible matters offered to it, the volatile parts it had loft.

This explanation appeared to simple and so clear to me, that I cannot perceive what can be opposed to it; the obscurity of chemistry proceeds in a great measure, because the principles have been but little generalized, and have not been united to those of physic. Chemists have adopted the affinities without comprehending them; that is to say, without understanding the connection of the cause to the effect, which, nevertheless; is no other than that universal attraction; they have created their phlogiston without knowing what it is, although

though it is fixed air and fire: they have formed in proportion as they have had need, of ideal things, mineralizers, mercurial earths, 'names and terms as much more vague as the acceptation is general. dare fay, that M. Macquer, and M. de Morveaux, are the first of our chemists who have began to speak French: this science, therefore, begins to increase fince they began to speak, and they will speak so much the better, and they will be the more eafily understood, as most of the technical words shall be banished, as they shall renounce all the little secondary principles drawn from method; as we shall be the more occupied to deduce them from the general principles of rational mechanism; and as we shall endeavour with greater care to bring them back to the laws of Nature, and that they more willingly facrifice the convenience of explaining, in a precarious manner, and according to art, the phænomena of the composition or decomposition of substances to the difficulty of presenting them for such as they are, that is to fay, for particular effects depending on general ones, which are the only true causes, the only real principles to which we must adhere if we would advance the science of natural philosophy.

I think I have demonstrated, that all the little laws of chemical affinities, which appeared so variable and so different among themselves, are, however, no other than the general law of the attraction common to all matter. That this great law, always constant and the same, appeared only to vary by its expression, which cannot be the same when the sigure of bodies enters, like an element, into their distance. With this new key we can unlock the most prosound secrets of Nature; we can attain the knowledge of the sigure of the primitive parts of different substances; assign the laws and degrees of their assimities; determine the forms which they take by re-uniting, &c. I think also, I have made

it understood how impulsion depends on attraction; and that, although it may be confidered as a different force, it is, notwithstanding, a particular effect of this fole and general force. I have represented the communication of motion as impossible any otherwife than by a fpring, from whence I have concluded, that all bodies in Nature are more or less elastic, and that there is not one perfectly hard; that is to fay, entirely deprived of a spring, fince all are susceptible of receiving motion. I have endeavoured to flew how this fole force may change direction, and the attraction become at once repulfive; and from these opposite principles, which are all founded on rational mechanics, I have endeavoured to deduce the principal operations of Nature, such as the production of light, heat, fire, and their action on different substances: this last object which interests us the most in a vast field, the cultivating of which supposes more than an age, and of which I can only cultivate a little fpot, by putting into more capable and laborious hands the instrument I made use of. These instruments are the three modes of making use of fire by its velocity, its volume, and its mass; by applying it concurrently to the three classes of substances, which all either lose or gain by the application of fire. The experiments which I have made on the cooling of bodies, on the real weight of fire, on the nature of flame, on the progress of heat, on its communication, deperdition, concentration, and its violent action without flame, &c. are also so many instruments which will spare those much labour who will make use of them, and will produce a very ample fund of useful know. ledge.

Of the E L E M E N T S.

PART II.

Of AIR, WATER, and EARTH.

[] E have seen that air is the necessary, adminicle, and the first food of fire, which can neither fubfift nor propagate, nor augment itself but as much as it affimilates, confumes, or carries it off; whereas, of all material substances, the air is, on the contrary, that, which appears to exist the most independently, and sublift the easiest and most constant, without the aid or the presence of fire; for, although it habitually has nearly the fame heat as the rest of the matters on the furface of the earth, it can do without it, and it requires infinitely less than all the rest to support its fluidity, fince the most excessive cold, whether natural or artificial, does not make it lofe any thing of its nature; that the strongest condensations are not capable of breaking its spring; that the active fire, or rather actually in exercise on combustible matters, is the only agent which can alter its nature by rarefying it; that is to fay, by weakening it, and extending its spring to the point of rendering it ineffectual, and of thus destroying its elasticity. In this state of too great expansion and extreme weakness of its spring, and in all the links which precede this state, the air is capable of reaffuming its elafticity, in proportion as the vapours of combustible matters which had weakened it, evaporate and separate from it. But if the spring has been totally weakened, and fo prodigiously extended, that it cannot bind nor re-instate itself, having lost all its elastic power, the air of the volatile part which was before a fixed substance that incorporate with other substances, and makes a constituent part of all those to which it unites by contact, or in which it penetrates by the affistance of heat. Under this new form it can no longer forfake the fire, but to unite like fixed matter to other fixed matters; and if there remains fome parts inseparable from fire, they then make a portion of this element serve it for a base, and are deposited with it in the substance they heat and penetrate together. This effect, which is manifested in all calcinations, is so much the more certain, and fo much the more fenfible as the heat is longer. Combustion demands but a little time to do the fame completely, whereas all calcination supposes much time; to accelerate it, we must bring it to the furface; that is to fay, present fuccessively to the air; matters we would calcine, those we must melt or divide them into impalpable parts, in order to offer a greater superficies to this air, we must even make use of bellows, not so much to augment the heat of the fire, as to establish a current of air on the furface of matters, if we would hasten their calcination; and to complete it with all these modes, much time is often required; from whence we must conclude, that there is also a pretty long refidence of the air become fixed in the terrestrial substances to establish its stay under this new form.

But it is not necessary for the fire to be very fierce to deprive air of its elasticity: the least fire, and even a very moderate heat, when it is immediately and constantly applied on a small quantity of air, is sufficient to destroy the spring; and for this air, without spring, to fix itself afterwards in bodies, there is only a little more, or a little less time required, according to the greater or lesser affinity it may have under this new form with the matters to which it unites. The heat of the body of animals, and even of vegetables, is still powerful enough to produce this effect: the degrees of heat are different in different kinds of animals; and, to begin with birds, which are the hot-

test of any, we pass successively to quadrupedes, man, and to cetaceous animals, which are less so; to reptiles, fish, and insects, which are still less; and at last to vegetables, whose heat is so trisling, as to have made fome naturalists declare them to have none at all, although it is very real, and in winter furpasses that of the atmosphere. I have observed on a great number of trees cut in cold weather, that their internal part was very fenfibly warm, and that this warmth remained for many minutes after they had been cut down. It is not the violent motion of the wedge, or the reiterated and brisk friction of the faw, which alone produced this warmth; for in afterwards flitting this wood with wedges, I have obferved, that it was but hot to two or three feet diftance from the part where the wedges had been placed, and that, confequently, it had a degree of heat pretty fensible in all its internal part. This heat is only very moderate while the tree is young and found; but as foon as it grows old, the heart heats by the fermentation of the pith, which no longer circulates there with the same freedom. This part of the center receives in heating a red tint, which is the first index of the perishing state of the tree, and the diforganization of the wood. I have many pieces in this state which were as hot as if they were heated by the fire. If naturalists have not found, that there was any difference between the temperature of the air and the heat of vegetables, it is because they have made their observations at a bad time of the year, and have not paid attention, that in the fummer the heat of the air is as great and greater than that of the internal part of a tree; whereas, in winter, it is quite the contrary: they have not remembered that the roots have conflantly at least the degree of heat which furrounds them; and that this heat of the internal part of the earth is during all winter confiderably greater than that of the the air, and the furface of the earth cooled by the air: they are not recalled, but the rays of the fun falling too brifkly on the leaves and other delicate parts of vegetables, not only heats, but burns them; that they heat likewise to a very great degree the bark and wood, the furface of which they penetrate and, which they extinguish and fix. They did not imagine, that the motion alone of the pith, already warm, is a necessary cause of heat; and that this motion, increasing by the action of the fun, or by an external heat, that of vegetables must be so much the greater as the motion of their pith is more accelerated, &c. I do not infift fo long on this point, than for the cause of its importance; the uniformity of Nature's plan would be violated, if, having granted to every animal a degree of heat superior to that of inanimate matters, it had refused it to vegetables, which, like animals, have their kind of life.

But here the air contributes still to the animal and vital heat, as we have feen that it contributes to the action of fire in the combustion and calcination of combustible and calcinable matters. Animals, which have changes, and which, consequently, respire air, have always more heat than those which are deprived of it; and the more the internal furface of the lungs is extended and ramified in a greater number of cells, the more, in one word, it presents a greater supersicies to the air which the animal draws by inspiration; the more also its blood becomes hotter, and the more it communicates heat to all parts of the body it nourishes, and this proportion takes place in all known animals. Birds, relatively to the volume of their body, have lungs confiderably more extended than man or quadrupedes. Reptiles, even those which have a voice, as frogs, have instead of lungs a fimple bladder. Infects which have little or no blood, pump the air only by some pipes, &c. Thus, by taking the degree of the temperature of the

earth for the term of comparison, I have observed. that this heat being supposed ten degrees, that of birds was nearly thirty-three, that of fome quadrupedes more than thirty-one and a half, that of man thirty and a half, or thirty-one; whereas, that of frogs is only fifteen or fixteen, that of fishes and infects eleven or twelve; that is to fay, the least of all, and very nearly the fame as that of vegetables. Thus the degree of heat in man and animals, depends on the force and extent of the lungs: these are the bellows of the animal machine; they support and augment the fire according as they are the more or less powerful, and their motion more or less ready. The only difficulty is to conceive how these kinds of bellows (whose construction is as superior to that of our common bellows as Nature is beyond art) may carry the air on the fire which animates us: a fire, whose focus feems to be indeterminate; a fire that has not even been qualified with this name, because it is without flame, without any apparent fmoke, and its heat only very moderate uniform. However, if we confider that heat and fire are the effects, and even the elements of the fame class: if we recollect, that heat rarefies air, and that, by extending its fpring, it may weaken it to the point of rendering it without effect. may imagine, that this air drawn by our lungs rarefy greatly there, must lose its spring in the bronchia and little veficles where it can penetrate only in a very fmall volume, and in bubbles whose spring is already very extended, will be foon destroyed by the arterial and venous blood; for these blood vessels are not separated from the pulmonary vesicles which receive the air, but by fuch thin divisions, that they eafily fuffer this air to pass into the blood, where it cannot fail of producing the fame effect as upon common fire; because the degree of the heat of this blood is more than sufficient to destroy entirely the elafficity elasticity of the particles of air, fixes and drags them under this new form into all the roads of circulation. The fire of the animal body does not differ from the common fire much, excepting the degree of heat is less: from hence there is no flame, because the vapours which are raifed up, and which represent the fmoke of this fire, have not heat enough to inflame or become ardent; and that being, befides, mixed with many humid parts which they take up with them, these vapours, or this smoke, can neither light nor burn, every other effect is absolutely the fame: the respiration of a young animal absorbs as much air as the light of a candle: in closed veffels, of equal capacities, the animal dies in the fame time as the candle extinguishes: nothing can more evidently demonstrate, that the fire of the animal and that of the candle, or of every other lighted combustible matter, are not only fires of the same class, but of one and the same nature, to which the affistance of the air is equally necessary, and which both appropriate in the same manner, absorb it like food, and carry it with them in their road, where they deposit it under a fixed form in the substances which they penetrate.

Vegetables and most insects have in the room of lungs, only aspiratory tubes or pipes, a kind of trachea's by which they pump up the air which is necessary for them; it is seen to pass in very sensible balls into the pith of the vine; it is not only pumped up by the roots, but often even by the leaves; it forms a part, and a very essential part, of the food of the vegetable which assimilates, sixes, and preserves it. The little degree of vegetable heat, joined to that of the sun's heat, is sufficient to destroy the spring of the air contained in the pith, especially when this air which has not been admitted into the body of the plant, nor arrived at the pith, till after having passed through very narrow pipes,

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is found divided into particles almost as infinitely small, and the least degree of heat suffices to fix them. Experience fully confirms all what I have just advanced. Animal and vegetable matters all contain a very great quantity of this fixed air; and it is in this wherein confifts all the principles of their inflammability. All combustible matters contain much air; all animals and vegetables; all their parts, and all their waste, all the matters which proceed therefrom, and all the substances or the waste found mixed therewith, contains more or less fixed air; and the greatest part also includes a certain quantity of elastic air. We cannot doubt of these particulars, the certainty of which is evinced by the great experiments of Dr. Hales, the due value of which chemists appear to me not to have properly fet, or else they would a long time ago have discovered, that fixed air must take great part of the place of their phlogiston, nor would they have adopted this new term which answers no precise idea, nor fixed the basis of all their explanations from chemical phænomena, they would not have given it for an identical matter always the fame, fince it is composed of air and fire, sometimes in a fixed state, and sometimes in that of a great volatility; and those among them who have regarded the phlogiston as the produce of elementary fire, or light, are less remote from the truth, because fire or light produces, with the affistance of air, all the effects of phlogiston.

Minerals, which like fulphur and pyrites contain in their fubstance a greater or less quantity of the ulterior waste of animals and vegetables, include from thence combustible matters, which, like all the rest, contain more or less fixed air, but always much less than the purely animal or vegetable substances. We can also drive from them this fixed air by combustion; we may also disengage by the mode

of effervescence; and in animal and vegetable matters, it is disengaged by simple fermentation, which, like combustion, has always need of air for its operation. This agrees fo perfectly with experience, that I imagine it not necessary to infift on the proof of facts. I shall content myself with observing, that fulphurs and pyrites are not the only minerals which must be looked upon as combustible; that there are many others of which I shall not here enumerate, because it is sufficient to say, that their degree of combustibility depends commonly on the quantity of fulphur which they contain. All combustible minerals, therefore, originally derive this property either from the mixture of animal or vegetable parts which are incorporated with them, or from the particles of light, heat, and air, which by the lapse of time are fixed in their internal part. Nothing, according to my opinion, is combustible, but that which has been formed by a gentle heat; that is to fay, by these same elements combined in all the substances which the sun brightens and vivifies, or in that which the internal heat of the earth foments and unites.

It is this internal heat of the globe of the earth that we must look upon as the true elementary fire; and we must distinguish it from that of the sun which comes to us only with light; whereas, the other, although much more considerable, is commonly under the form of an obscure heat; and it is only in some circumstances, as those of electricity, that it receives light. We have already observed, that this heat, taking for a whole year, and during a great number of years successively, is three or four hundred times greater than the sum of the heat which comes to us from the sun during the same time: it is a truth which may appear singular, but which is not evidently demonstrated. As we have spoken on this subject, we shall content ourselves here

with remarking, that this conftant, and always fubfifting heat, enters like an element into all the combinations of the other elements, and that it is more than sufficient to produce the same effects on air as actual fire on animal heat; that, consequently, this internal heat of the earth will destroy the elasticity of the air, and will fix it every time; that being divided into very minute parts, it shall be found feized by this heat in the bowels of the earth; that under this new form it will enter as a fixed part into a great number of substances, which will from hence contain the particles of fixed air and fire, which are the first principles of combustibility: but they will be found in a greater or less quantity in different substances according to the degree of affinity that they will have with them, and this degree will depend much on the quantity that these substances shall contain of animal and vegetable parts, which appear to be the base of all combustible matter. If they are abundantly diffused therein, and weakly incorporated, we may always disengage them from these fubitances by the mode of combustion. Most of the metallic minerals, and even metals, contain a fufficient great quantity of combustible parts; zink, antimony, iron, copper, &c. burn and produce an evident and very brisk flame, as long as the combuttion of these inflammable parts remain, after which, if the fire is continued, the combustion finishes, and the calcination begins, during which there enters into these matters new parts of air and heat which fixes there, and which cannot be difengaged but by offering them some combustible matter with which these parts of fixed air and heat have a greater affinity than with those of the mineral, with which, in fact, they are only united by strength; that is to fay, by the effort of calcination. It feems to me, that the conversion of metallic substances into drofs, and their reduction might now be very clearly understood, without the need of running to secondary principles or arbitrary hypothesis's for their explanation. The reduction, as I have already infinuated, is in reality only a second contraction, by which the parts of the air are disengaged, and heat fixed, which calcination had forced to enter into the metal, and of uniting itself with its fixed substance, to which, at the same time, is rendered the volatile and combustible parts that the first action

of the fire had taken away.

After having presented the action of fixed air in the most secret operations of nature, let us consider it for a few moments when it resides in bodies under an elastic form: its effects are then as variable as the degrees of its elasticity; its action, though always the same, seems to give different products in different substances. To bring this consideration back to a point of general view, we will compare it with water and earth, as we have already compared it with fire; the results of this comparison between the four elements will afterwards be easily applied to every substance, of whatever nature they may be of, since they are all only composed of these four real principles.

The greatest cold that is known, cannot destroy the spring of the air, and the least heat is sufficient for that purpose, especially when this sluid is divided into very minute parts. But it must be observed, that between its state of fixity, and that of its persect elasticity, there is all the links of the mediate states, and that it almost always is in some one of these mediate states, that it resides in earth and water, and all the substances which are composed of them; for example, it cannot be doubted but that water, which appears to us so simple a substance, contains a certain quantity of air, which is neither fixed nor elastic, but that fixity and elasticity enters it, if we consider the different phænomena which it presents us

in its congelation, ebullition, and its refiftance to all compression, &c. for experimental physic demonstrates to us, that water is incompressible, instead of shrinking and entering into itself when pressed, it paffes through the most folid and the thickest vessels. Now, if the air which it contained in a pretty large quantity, was in its state of full elasticity therein, water would be compressible, by reason of this quantity of elastic air, which it contains, and which it compresses; the air contained therefore in water, is not fimply mixed therewith, and is not perceived then in its elastic form, but it is more intimately united there in a state where its spring is not exercised in a fenfible manner; yet this fpring is not entirely destroyed there, for if we expose water to congelation, we fee this air iffue from its internal part, and unite on its furface in elastic bubbles. This alone suffices to prove, that air is not contained in water under its common form, fince being specifically eight hundred and fifty times lighter, it would be forced to iffue out by the fole necessity of the preponderance of water. It is therefore evident, that the air contained in water, is not in its common flate therein; that is to fay, of full elafficity, and at the same time it is demonstrable, that this state in which it resides in water, is not that of its greatest fixity, or its spring absolutely destroyed, cannot be established but by combustion, fince heat or cold may be alike established. It is fufficient to congeal water for the air it contains to re-take its elafficity, and rife up in fensible bubbles to its furface; it disengages itself likewise, when the water ceases from being pressed by the weight of the atmosphere under the recipient of the pneumatic machine; it is not therefore contained in water under affixed form, but only in a medium state, where it can eafily re-take its fpring: it is not fimply mixed in water, fince it cannot refide there under its elastic form, but also it is not intimately united with

with it under its fixed form, fince it feperates more

eafily than from every other matter.

It may be objected, and with reason, that cold and heat have never operated in the fame mode; that if one of these causes renders air its elasticity, the other must destroy it, and I own that in general, cold and heat produce different effects. But in the particular fubstance that we shall confider these two causes, although opposite, produce the same effect. It may eafily be conceived, by paying attention to the thing itself, and to the connection of its circumstances. is known that water, frozen or boiled, retakes the air it had loft as foon as it is liquified or cooled. The degree of affinity of air with water, depends therefore in a great part from its temperature; this degree in its liquid state, is nearly the same as that of the general heat to the furface of the earth: the air with which it has much affinity penetrates it as foon as it is divided into fmall parts, and the degree of elementary and general heat, fuffices to weaken the fpring of these parts, to the point of rendering them ineffectual, as long as the water preferves this temperature; but if the cold penetrates it, or to speak more precifely, if this degree of heat, necessary to this state of the air diminishes, then its spring, which is not entirely deftroyed, will be re-established by the cold, and we shall perceive the elastic bubbles rife to the furface of the water ready to freeze; if, on the contrary, the temperature of the water is increased by an external heat, the integrant parts are too much divided, they are rendered volatile, and the air which was united with them but flightly rifes and escapes with them; for it must be recollected, that although water taken in a glass is incompressible and without any fpring, it is very elastic as foon as it is divided or reduced into fmall parts; and in this it appears to be of a nature contrary to that of the air, which is compressible but in a mass, and which loses its spring when

when it is too greatly divided. However, water and air have much greater connections between them than opposite properties, and as I am very well persuaded, that all matter is convertible, and that the elements may be transformed, I should be inclined to believe, that water can change itself into air when it is enough rarefied to raise itself up into vapours, for the spring of the vapour of the water is as, and even more powerful than the spring of the air. We see the prodigious effect of this power in sire pumps; we see the terrible explosion that it produces, when melted metal falls on some drops of water, and if they do not chuse to agree with me, that water in this state of vapours is transformed into air, it cannot at least be denied but that it then has the principal properties.

Experience has even taught me that the vapours of water can increase the fire as common air does; and this air, which we should regard as pure, is always mixed with a very great quantity of water; but it must be remarked as an important thing, that the proportions of the mixture is not nearly the fame in these two elements. It may be faid in general, that there is much less air in water than water in air: it must be only considered that there is two different units, to which we might refer the terms of this proportion, these two units are the volume and mass. If we estimate the quantity of air contained in water by the volume it will appear nil, fince the volume of water is not at all increased; and the same likewife, the more or less moist air does not appear to us to change the volume: this only happens when it is more or less hot. Thus, it is not to the volume that we must relate this proportion, it is alone to the mass; that is to fay, to the real quantity of matter in one and the other of these two elements, that we must compare that of their mixture, and we shall perceive that the air is much more aqueous than the water is erial, perhaps in proportion of the mass, that is to

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fay, eight hundred and fifty times more. Be this estimation as it will, which is perhaps either too firong or too weak, we can derive this induction from it, that water must change more easily into air, than air can transform into water. The parts of air, although fusceptible of being extremely divided, appear larger than those of water; fince this paffes through many fibres, which air cannot penetrate: fince, when it is rarified by heat, its volume, although very much increased, is only equal or a little less than that of the parts of air to the surface of the earth; for the vapours of water are only raised to a certain heigth in the air. In thort, fince air feems to imbibe water like a spunge; to contain it in a large quantity, and that the container is certainly greater than the contained. On the whole, air, which so readily imbibes water, seems likewise to be rendered back when falts or other fubstances with which water has a still greater affinity are offered to it. The effect called by the chemists decantation, and even that of efficience, not only demonstrates that there is a very great quantity of water contained in air, but also that this water is upheld there by a simple affinity. which cedes easily to a still greater assinity, and which even ceases to act without being counterbalanced or overcome by any other affinity, except the refraction alone of the air; fince it disengages itself from water, as iron as the weight of the atmosphere ceases from pressing it, under the recipient of the pneumatic machine.

In the order of the conversion of the elements. it appears that water is to air what air is to fire, and that all the transformations of nature depends on them: air like the food of fire assimilates with it. and is transformed into this first element. Water rarified by heat, is transformed into a kind of air capable of feeding the fire like common air. Thus, fire has a double fund of certain subfistence; if it VOL. V.

confumes much air, it can also produce much by the rarefaction of water, and thus repair in the mass of atmosphere all the quantity which it destroyed, while ulteriorly it converts itself with air into fixed matter in the terrestial substances which it penetrates by its heat or by its light. And so likewise as from one part water is converted into air or into vapours, as volatile as air by its rarefaction; it is converted into a folid substance by a kind of condensation different from common condensations: every fluid is rarefied by heat, and condensed by cold. Water itfelf follows this common law, and condenses in proportion as it grows colder. Let a glass tube be filled three parts full, we shall find it descend in proportion as the cold increases and condenses like all other fluids: but, some time before congelation, we shall see it ascend above the point of three fourths of the height of the tube, and increase still more considerably by being frozen; but if the tube is well stopped and perfectly at rest, the water will continue to lower and will not freeze, although the degree of cold is fix, eight or ten degrees below the freezing point, nor will the water freeze when the tube is opened or shaken. It seems therefore that congelation prefents to us in an inverted manner the same phænomena as inflammation. However intense, however great a heat thut up in a well-closed vessel is, it will not produce inflammation, but when it shall be touched with an inflamed matter, and so likewise to whatfoever degree a fluid is cooled, it will not freeze unless it touches something already frozen, which is what happens when we shake or unstop the tube. The particles of water which are frozen in the external air, or in the air contained in the tube. when it is shaken or unstopped, strikes the surface of the water and communicates their ice to it. In inflammation, the air at first very much rarefied by heat, loses its volume and fixes itself suddenly. In congelation, water at first condensed by the cold, takes a larger volume, and fixes itself likewise; for ice is a solid substance lighter than water, and would preserve its solidity if the cold was always the same: and I am inclined to believe, that we should attain the point of fixing mercury at a less degree of cold, by sublimating it into vapours in a very cold air; so likewise I am inclined to believe, that water, which only owes its liquidity to heat, and which loses it with it, would become a substance much more solid and suspenses it would endure a stronger and a longer time the rigour of the cold. Experiments enough have not been made on this important sub-

ject.

But without stopping on this idea, that is to fay, without admitting or excluding the possibility of the conversion of the ice into infusible matter, or fixed and folid earth, let us pass on to more extensive views on the modes Nature makes use of for the The most powerful and transformation of water. the most evident of all is the animal filter. The body of shell animals, by feeding on the particles of water, labours, at the fame time, on the fubstance to the point of unnaturalizing it. The shell is certainly a terrestrial substance, a true stone, from which all the stones that the chemists call Calcareous, and many other matters, derive their origin. This shell, in fact, appears to make the constitutive part of the animal it covers, fince it is perpetuated by generation; and we fee it on the fmall shell animals just born, as on those which have taken their full growth: but this is no less a terrestrial substance, formed by the fecretion or exudation of the body of the animal; it is feen to increase and thicken by rings and layers, in proportion as it grows; and often this stony matter exceeds fifty or fixty times the mass or real matter of the animal's body which produces it. Let us for a moment represent the number of the kinds of shell animals; or, to comprehend it better, of those animals with a stony transudation; they, possibly, are more numerous in the fea, than the infect kind are upon earth. Let us afterwards represent their full growth, their prodigious multiplication, and the shortness of their life, which we shall suppose to be ten years: let us afterwards confider, that we must multiply by fifty or fixty the almost immense number of all the individuals of this class, to form an idea of all the stony matter produced in ten years: let us at length confider, that this block, already fo large with stony matter, must be augmented with as many fimilar blocks as there are as many times ten in all the ages from the beginning of the world; and we shall familiarize ourselves by this idea, or at first repulsive truth, that all our coral, all our rocks of calcareous stone, marble, chalk, &c. proceed originally only from the cast off coats of those little animals. We cannot doubt, even by the inspection of matters, that all contain shells, or the waste of shells, very eafily distinguishable.

Calcareous stones are, therefore, in a very great part only water and air contained in water, tranfformed by the animal filter. Salts, bitumen, oil. and the greafe of the fea, enter little or none into the composition of the shell. The calcareous stone also contains none of these matters: this stone is only water transformed, joined to fome little portion of vitrifiable earth, and a very great quantity of fixed air, which is difengaged therefrom by calcination. This operation produces the same effects on the thells taken in the fea, as upon the fhells drawn out of quarries: they both form lime, in which no other difference is remarked, than that of a little more or a little less quality. Lime made with oyster or other thells, is weaker than the lime that is made with hard flone; but the process of Nature is the same, as are the refults of its operation. Shells and stones alike lose nearly half their weight by the action of fire in calcination: the water which has preferved its nature, iffues the first, after which the fixed air is difengaged; and then the fixed water, of which these stony substances are composed, retakes its first nature, and is elevated into vapours drove off and rarefied by the fire, there remains only the most fixed parts of this air and water, which, perhaps, are fo strongly united in themselves, and to the small quantity of the fixed earth of the stone, that the fire cannot separate them. The mass finds itself, therefore, reduced nearly an half, and would, perhaps, be reduced still more, if a fiercer fire was given to it. And what appears evidently to prove to me, that this matter, driven out of the stone by the fire, is nothing elfe than air and water, is the rapidity, or avidity, with which this calcined stone drinks up the water given to it, and the force with which it draws it from the atmosphere when it is refused it. Lime, by its extinction either in air or water, in a great measure regains the mass it had lost by calcination: the water, with the air it contains, replaces the water and the air it contained before; the stone then retakes its first nature; for in mixing lime with the remains of other stones, a mortar is made which hardens, and by time becomes a folid and stony substance, like those from which it is composed.

After this exposition, I think we cannot doubt of the transformation of water into earth, or stone, by intermediate shells. Thus, then, we see on the one hand all the calcareous matters, the origin of which we must refer to animals; and, on the other, all the combustible matters proceeding from animal or vegetable substances; they together occupy a great space on the earth, and by their immense volume it may be judged how greatly living Nature has laboured for

the dead; for the inanimate is in this place no other than dead matter.

But calcareous matters and combustible substances. however great the number may be, only make a very small portion of the terrestrial globe, the principal foundation of which, and the greater or greatest quantity confists in one matter of the nature of glass; a matter we must look upon as a terrestrial element, to the exclusion of all other substances to which it ferves as a base, like earth, when it forms vegetables by the means or remains of animals, and by the transformation of the other elements. Not only this first matter, which is the true elementary earth, ferves as a base to every other substance, and conflitutes the fixed parts thereof; but it is, at the fame time, the ulterior term to which we can return or reduce them all. Before we prefent the means Nature and Art makes use of to perform this kind of reduction of every substance into glass, that is to fay, into elementary earth, it is right to fearch whether the modes we have indicated are the only ones by which water can be transformed into a folid fubstance: it appears, that the animal filter converting it into stone, the vegetable filter can also transform it, when all the circumstances are found to be the same. The heat of shell animals being fomewhat greater than that of vegetables, and the organs of life more powerful than those of vegetation, the vegetable cannot produce but a small quantity of stones found often enough in its fruit; but it can and does convert really a great quantity of air, and a still greater quantity of water into its fubstance. The fixed earth it appropriates, and which ferves as a base to these two elements, is in so fmall a quantity, that we can affert, without the leaft fear of deceiving, that it does not make the hundredth part of its mass: hence, the vegetable is almost entirely composed of only air and water, transformed

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formed into wood, or a folid substance which is afterwards reduced into earth by combustion and putrefaction. We must say the same of animals; they not only fix and transform air and water, but fire, in a much greater quantity than vegetables. It appears, therefore, that the functions of organized bodies are one of the most powerful means made use of by Nature for the conversion of the elements. We may look on each animal, or vegetable, as a small particular center of heat or fire which appropriates to itself the air and water which surround it, assimilates them to vegetate or nourish, and live on the productions of the earth, which are themselves only air and water precedently fixed: it appropriates to itself, at the fame time, a small quantity of earth, and receiving the impressions of the light, and those of the heat of the fun and terrestrial globe, it converts into its substance all these different elements; works. combines, unites, and opposes them, till they have undergone the necessary form towards its unfolding; that is to fay, the support of life, and the growth of organization, the mold of which once given, models' every matter it admits, and from inanimate renders it organized.

Water, which so readily unites with air, and which enters with it in such a great quantity into organized bodies, unites also in preference with some solid matters, such as salts; and it is often by their means that it enters into the composition of minerals: salt at first appears to be only an earth soluble in water, and of a sharp savour: but chemists, in looking into its nature, have perfectly discovered, that it principally consists in the union of what they term the earthy principle, and the aqueous principle. The experiment of the nitrous acid, which after combustion only leaves a small quantity of earth and water, has even caused them to think, that perhaps this, and every other salt, was absolutely composed



only of these two elements; yet, it appears to me eafily demonstrable, that air and fire enters their composition; fince nitre produces a great quantity of air in combustion, and that this fixed air supposes fixed fire which disengages at the same time: that besides all the explanations given of the dissolution cannot be supported, at least, that they do not admit two opposite powers, the one attractive and the other expansive; and, consequently, the presence of the elements of air and fire, which are folely endowed with this fecondary power; that, in short, it would be against all analogy, that falt should be composed only of these two elements, while all other fubstances are composed of four. Thus, we must not take for granted what those great chemists, Meffrs. Stahl and Macquer have faid on this subject. The experiments of M Hales demonstrates, that vitriol and marine falt contain much fixed air; that nitre contains still much more, even to the eighth of its weight; and that falt of tartar contains still more We may, therefore, affirm, that air than those. enters as a principle into the composition of all salts; and as it cannot fix itself into any substance without the aid of heat, or fire, which fixes itself at the same time, they must be reckoned among the number of their constitutive parts. But this does not prevent falt being also not regarded as the mediate substance between earth and water; these two elements enters in different proportions into the different falts, or faline substances, whose variety and number are so great, as not to be enumerated; but which, generally presented under the denominations of Acids and Alkalis's, shews us, that there is in general more earth and less water in these last falts; and, on the contrary, more water and less earth in the first.

Nevertheless water, although intimately mixed with falts, is neither fixed nor united there by a force great

great enough to transform into a folid matter, as in the calcareous frome : it renders it falt or acid under. its primitive form, and the best concentrated acid. or the most deprived of water, which might be looked upon here as liquid earth, only owes this liquidity to: the quantity the air and fire it contains, All liquis dity and even all stuidity, supposes the presence of a certain quantity of fire; and when we would attribute that of acids to a small portion of water, which could not be seperated from it; when even we could reduce them under a concrete form, it would not be less certain that this favour, as well as their odours and colours, have like a principle the expanfive power, that is to fay the light, and the emotions of heat and fire, for there is only three active principles; which can act upon our fenfes, and affect them in a different and diverlified manner according to the vapours or particles, of the different fubftances they bring and prefent to us. It is therefore to these principles we must refer, not only the liquidity of acids but also their favour. An experiment which I have had occasion to make severaltimes, has fully convinced me, that alkali is produced by fire. Lime made according to the common mode, and put upon the tongue even before flacked by air or water, has a favour which already indicates the presence of a certain quantity of alkali. If the fire is continued, this lime, which has undergone a longer calcination, becomes more poignant to the tongue; and that drawn from furnaces, where the calcination has fublifted for five or fix months together, is fill more to. Now, this fait was not contained in the stone before its calcination; it augments in strength or in quantity, in proportion as the fire is applied fiercer and longer to the stone: it is therefore the immediate product of the fire and air, which incorporates in the substance during its calcination; and which by this means, are become fixed parts of this VOL. V. E ftone:

stone; from which they have driven most of the watry liquid and solid molecules it before contained. This alone appeared to me sufficient to pronounce that fire is the principle of the formation of the mineral alkali, and we must conclude, by analogy, that other alkali's alike owe their formation to the constant heat of the animal and vegetable from which we draw them.

With respect to acids, the demonstration of their formation by fire and fixed air, although less immediate than that of alkalis, does not appear less certain. We have proved, that nitre and phosphorus drew their origin from vegetable and animal matters; that vitriol derives its origin from the pyrites, fulphur, and other combustibles: it is likewise known, that these acids, whether virriolic, nitrous, or phosphorical, always contain a certain quantity of alkali; we must, therefore, refer their formation and their favour to the same principle, and reducing all acids to a fingle one, and every alkali to one alkali, bring back all falts to one common origin, and not look on their different favours, and their particular and diverfe properties, only as the varied product of the different quantities of earth, water, and especially air and fixed fire, which enter into their composition: those which will contain most of these active principles of air and fire, will be those which will have most power and most taste. I understand by power the force with which falts appear animated to disfolve other substances. It is well known, disfolution supposes fluidity, that it never operates between two dry or folid matters, and, confequently, it also supposes the principle of fluidity in the dissolvent; that is to fay, fire: the power of the diffolvent will be, therefore, fo much the greater, as on one part it contains this active principle in a greater quantity; and that, on the other hand, its aqueous and terrene parts will have more affinity with the parts

parts of the fame kind contained in the substances to diffolve; and as the degrees of affinity depend abfolutely on the figure of the integrant parts of the body, they must, like those figures, vary ad infinitum : we must, therefore, not be surprized of the greater, leffer, or no action of certain falts on certain fubstances, nor of the contrary effects of other salts on other substances: their active principle is the same, their diffolving power the same; but it remains without exercise when the substance presented it, repels that of the diffolvent, or has no degree of affinity with it; while, on the contrary, it greedily lays hold of it every time it finds sufficient force of affinity to conquer that of the coherence; that is to fay, every time that the active principles, contained in the dissolvent, under the form of air and fire, is found more powerfully attracted by the fubstance to be diffolved, than they are by the earth and water they contain; for from hence these active principles separate, develope, and penetrate the substance they divide and decompose to the point of rendering it fusceptible, by this division, to freely obey all the attractive forces of earth and water contained in the diffolvent, and to unite with them intimately enough not to be separated therefrom but by other substances which might have a still greater affinity. Newton is the first who has given affinities for the causes of chemical precipitation: Stahl adopted this idea, and transmitted it to every chemist; and it appears to me to be at present universally received as a truth which cannot be doubted: but neither Newton nor Stahl faw that all these affinities, so different in appearance among themselves, are at bottom only particular effects of the general force of universal attraction; and, in defect of this view, their theory can neither be luminous nor complete, because they were obliged to suppose so many trivial laws of different affinities, as there were different phænomena; phænomena; whereas, there is really only one fingle law of affinity, a law exactly the same as that of universal attraction; and that, consequently, the explanation of every phænomena must be deduced from this sole cause.

Salts, therefore, concurs in many operations of Nature by the power they have of diffolving other fubstances: for, although it is vulgarly said, that water diffolves falt, it is easy to be perceived, that it is a wrong expression, founded on what the liquid is commonly called, the diffolvent, and the folid, the diffolving body; but, in reality, when there is a diffolution, both bodies are active, and may be alike called diffolvents: regarding only the falt as diffolvent, the dissolved body may be indifferently either liquid or folid; and, provided that the parts of the falt be fufficiently divided to touch immediately those of other substances, they will act and produce all the effects of diffolution. By this we fee how much the proper action of falts, and the action of the element of water which contains them, must have influence on the composition of mineral matters; Nature may produce by this mode, all that our arts produce by the mode of fire. Time only is required for falts and water to operate on the most compact and the most hard substances, the most complete division, and the greatest attenuation of their parts, which then renders them susceptible of all possible combinations, and capable of uniting with all analogous substances, and to separate from all others: but this time, which is nothing to Nature, and which is never wanting to it, is, of all things which are necessary, that which is the most deficient to us: it is the defect of time which prevents us from imitating her processes, or following her track : the greatest of all our arts, therefore, would be the art of abridging the time; that is to fay, of performing in one day what the does in an age, However vain

this pretention may appear, we must not renounce it. We have in fact, neither the great force, nor the still greater time of nature: but we have more than her, the liberty of using them as we please. Our will is a force which commands all the rest, when we direct it with judgment. Have we not attained the point of creating to our use, the element of sire which has been hidden from us? have we not attracted the rays which she has sent to enlighten us? have we not by this same element found the means of abridging time, by dividing bodies by a suspense sudden as this division would be slow by any other

mode, &c. But this must not make us lose fight that nature cannot, nor does not really perform, by the means of water, all that we do by fire. To fee this clearly, we must consider that the decomposition of every fubstance not being able to be made but by division, the greater this division will be, and the more the decomposition will be complete. Fire seems to divide as much as possible, matters it puts in fusion; nevertheless it may be doubted whether those which water and acids keep in diffolution are not still more divided, and the vapours raifed by heat, do not they contain matters which are still further attenuated? In the bowels of the earth then, by the means of the heat it includes and the water which infinuates, there is made an infinity of fublimations, distillations, cirtallifations, agregations and disjunctions of every kind. By time all substances may be compounded and decompounded by thefe means; water may divide them and attenuate the parts fo much and more than fire when it melts them, and those attenuated parts divided to this point, will join and unite in the same manner, as those of fused metal unite by cooling. To make this more explicit, let us dwell for a moment on cristallisation. This effect, of which the falts have given us an idea, is never performed but when a substance being difengaged from every other substance is found very much divided and fustained by a fluid, which having little or no affinity with it permits it to unite and form, by virtue of its force of attraction, masses of a figure nearly fimilar to its primitive parts. This operation which supposes all the circumstances I have just announced, may be done by the intermediate aid of fire, as well as by that of water; and is very often made by the concurrence of both, because all this does not suppose or exact but one division of matter sufficiently great, for its primitive parts to be able, as I may fay, to cull and form, by uniting figured bodies like themselves. Now fire can quite as well, and much better than any other diffolvent, bring many substances to this state and observation demonstrates to us in asbestos, basaltes, and other productions of fire, whose figures are regular, and which must all be looked upon as true criftallifations.

And this degree of the great division necessary to cristallisation, is not yet that of the greatest possible, nor real division, since in this state the small parts of matter are still sufficiently large to constitute a mass which, like all other masses, only is obedient to the sole attractive force and the volumes of which only touching in points, cannot acquire the resultive force that a much greater division might not fail of performing by a more immediate contact, and this is also what we see happen in effervescences, where at once heat and light are produced by the mixture of two cold liquors. This degree of the division of matter is here much below the necessary degree for cristallisation, and the operation is also as rapidly made as the other is slowly.

Light, heat, fire, air, water and falts, are steps by which we descend from the top of Nature's ladder to its bate, which is fixed earth. And these are at the fame time, the only principles, that we must admit and combine for the explanation of all phenomena. These principles are real, independent of all hypothesis and all method: their conversion, their transformation is also as real, fince it is demonstrated by experience: It is the fame with the element of earth, it can convert itself, by volatilizing and taking the form of the other elements, as there takes that of earth in fixing: but in the fame manner as the primitive arts of fire, air, or water, form only bodies, or shapes? which may be looked upon as fire, air, or pure water. So likewise it appears to me quite useless to seek for a substance of pure earth in terrestial matters; fixity, homogeneity, the transparent luftre of the diamond has dimmed the fight of our chemists, when they have given this stone for pure and elementary fire; it might also be faid with as much and as little foundation, that it is pure water, all the parts of which are fixed to compose a folid diaphanous fubstance, like that; these ideas would not have been started, if it had not been supposed, that the terrene element has not more privilege of absolute fimplicity, than the other elements. That even, as it is the most fixed of all, and consequently the most conflantly passive, it receives as base all the impresfions of the rest, it attracts, admits and unites them, it incorporates, follows and fuffers them to be hurried away by their motion; and confequently it is neither more fimple, nor less convertible than the rest. The large maffes should be alone considered when we would define Nature: these elements have been well taken notice of by even the most ancient philofophers. The fun, atmosphere, earth and fea, &c. are the great masses on which they have established them, if there ever existed a planet of phlogiston, an atmosphere of alkali, an ocean of acid, and a mountain of diamonds, they might then be looked upon as the general and real principles of all bodies; but, on the contrary, they are only particular substances, produced like all the rest, by the combinations of

true elements.

In the great mass of solid matter, which earth represents to us, the superficial matter is the least pure earth. All matters deposited by the sea in form of fediment, all stones produced by shell animals, all fubstances composed by the combinations of the waste of the animal or vegetable kingdom: all those which have been changed by volcanean fires, or fublimed by the internal heat of the globe, are mixed and transformed substances; and although they compose very great masses, they do not clearly enough reprefent to us the clement of earth: they are vitrifiable matters whose mass is one hundred thousand times more confiderable than other subflances, which should be regarded as the true bafis of this element: at the same time, it is those which are composed of the most fixed earth, those which are the most ancient, and, nevertheless, the least changed: it is from this common foundation. all other fubstances have derived the basis of their folidity; for all fixed matter, ever so much decomposed, reduces ulteriorly into glass by the sole action of fire: it retakes its first nature when it is disengaged from fluid or volatile matters which were united with it : and this glass, or vitrious matter, which composes the mass of our globe, so much the better represents the element of earth, as it has neither colour, odour, taste, liquidity, nor fluidity, qualities which all proceed from the other elements, or belong to them.

If glass is not precisely the element of earth, it is at least the most ancient substance thereof, metals are more recent and less noble: most other minerals form within our fight. Nature produces mass only in the particular focus of its volcanos; whereas, every day she forms other substances by the combination

of glass with the other elements. If we would form to ourselves a just idea of her processes in the formation of the globe, which demonstrates to us, that it has been melted or liquified by fire: confider afterwards, that from this immense degree of heat, it has fuccessively passed to the degree of its actual heat: that in the first moments, where its furface has began to take confistence, inequalities must be formed, such as we see on the surface of melted matters grown cold: that the highest mountains, all composed of vitrifiable matters, exists, take their date from this moment, which is also that of the great maffes of air, water, and earth ! that afterwards, during the long space of time which its cooling supposes; or if we chuse the diminution of the heat of the globe to the point of actual temperature, there is made in these mountains, which were the parts most exposed to the action of external causes. an infinity of fulions, fublimations, aggregations, and transformations of all kinds, by the fire of the fun. and all the other causes which this great heat rendered more active than they at present are; and that, consequently, we must refer back to this date the formation of metals and minerals we find in great maffes, and in thick and continued veins. The violent fire of inflamed earth, after having raised up and reduced into vapours all what was volatile; after having driven off from its internal parts the matters which compole the atmosphere and fea, has at the same time sublimed all the least fixed patts of the earth, raifed them up, and deposited them in every void space, in all the cavities which formed on the furface in proportion as it cooled: this, then, is the origin and the gradation of the fituation and formation of vitrifiable matters, which fire has divided, formed, and fublimed. Ino bomnot med over , sooit

After this first establishment (and which still subfists) of vitrifiable matters and minerals into a great Vol. V. F mass, mass, which can be attributed to the action of fire alone. Water, which till then formed with air only a vast volume of vapours, began to take its actual state as soon as the superficies of the globe was cooled fufficiently not to repel nor diffipate them into vapours: it collected, therefore, and covered the greatest part of the surface of the earth, on which, finding themselves agitated by a continual flux and reflux, by the action of winds and heat, it began to act on the works of fire. It changed by degrees the fuperficies of vitrifiable matters, it transported the wreck of them, deposited them in form of sediments, it nourished shell animals, it collected their shells, produced calcareous stones, formed hills and mountains, which afterwards drying, received in their cavities all the mineral matters they could diffolve or take in.

To establish a general theory on the formation of minerals, we must then begin by distinguishing with the greatest care, first, those which have been produced by the primitive fire of the earth while it was burning with heat; fecondly, those which have been formed from the waste of the first by the means of water; and, thirdly, those which in volcanos, or other conflagrations posterior to the primitive fire, have a fecond time undergone the proof of a violent heat: these three objects are very distinct, and comprehend all the universal kingdom: by not losing fight of them, and by connecting there each mineral fubstance, we can scarcely be deceived in its origin, and even on the degrees of its formation. All mines which are found in maffes, or large veins, in our high mountains, must be referred to the sublimation of primitive fire: all those, on the contrary, which is found in little ramifications, in threads, in vegetations, have been formed only from the wafte of the first, hurried away by the stillation of waters. We fee evidently, by comparing, for example, the matter of the iron mine of Sweden with that of our iron mines in grain; these are the immediate work of water, and we see them formed before our eyes; they are not attracted by the load stone; they do not contain any fulphur, and are found only dispersed in the earth; the rest are all more or less sulphureous. all attracted by the load-stone, which alone supposes that they have undergone the action of fire: they are disposed in great hard and solid masses; their substance is mixed with a great quantity of asbestos, another index of the action of fire. It is the fame with other metals; their ancient foundation comes from fire, and all their great masses have been united by its action; but all their cristallifations, vegetations, granulations, &c, are due to the fecondary causes, or to water in the greatest part. I here limit my reflections on the conversion of the elements, because it would be anticipating on those which each mineral substance particularly exacts, and which will be best placed in the articles of the Natural History of Minerals,

REFLECTIONS on the LAW of ATTRACTION.

THE motion of the planets in their orbits, is a motion composed of two forces; the first is a force of projection, the effect of which would be exercised in the tangent of the orbit, if the continued effect of the second ceased one moment; this second force tends towards the sun, and by its effect would precipitate the planets towards the sun, if the first force in its turn ceased one single moment.

The first of those forces may be looked upon as an impulsion, whose effect is uniform and constant, and which has been communicated to the planets from the formation of the planetary system: the second may be considered as an attraction towards the sun,

and must measure on all the qualities which part from a center, by the inverted motion of the square of the distance; as, in fact, we measure the quantity of light, smell, &c. and all other quantities or qualities which propagate in a strait line, and are connected to one center. Now, it is certain, that attraction is propagated in a strait line, since there is nothing more strait than a plumb line, which falling perpendicularly to the surface of the earth, it tends directly to the center of the force, and goes very little out of the radius to the center: therefore, we may say, that the law of attraction must be the inverted ratio of the square of the distance, only because it parts from a center, or tends to it, which comes to the same.

But as this preliminary reasoning, however well founded I think it, might be contradicted by people who take little care of the force of analogies, and who are accustomed to render themselves up only to mathematical demonstrations. Newton thought it better to establish the law of attraction by phanomena, than by any other mode; and he has, in fact, geometrically demonstrated, that if many bodies move in concentrical circles, and that the squares of the time of their revolutions are as the cubes of their distances to their common center, the centripetal forces of their bodies are reciprocally as the square of the distances; and that if bodies move in orbits little different from a circle, these forces are also reciprocally as the fquare of the distances, provided that the apsides of these orbits are immoveable; thus, the forces by which the planets tend to the centers of their orbits, follow, in fact, the law of the square of the distance; and the gravitation being general and universal, the law of this gravitation is constantly that of the inverted ratio of the square of the distance; and I do not imagine that any perion doubts of the law of Kepler, and that it can be denied,

denied, that this is not fow ith respect to Mercury, Venus, the Earth, Mars, Jupiter, and Saturn, especially by confidering them apart, and as not being able to trouble one another, and by paying attention only to their motion round the sun.

Every time, therefore, that we shall consider only one planet, or one fatellite, moving in its orbit round the fun or some other planet, or that we shall have only two bodies both in motion, or one at rest and the other in motion, we may be affured, that the law of attraction exactly follows the inverted ratio of the square of the distance; fince, by all observations, the law of Kepler is found to be true, as much in regard to the principal planets, as to the fatellites of Saturn and Jupiter. Nevertheless, an objection might be here drawn from the motions of the moon. which are so far irregular, that Mr. Halley calls it, Sidus Contumax, and principally from the motions of its apsides, which are not immoveable, as geometrical supposition requires, on which is founded the refult which has been discovered of the inverted ratio of the square of the distance for the measure of the force of attraction in the planets.

There are many modes of answering this; at first it might be said, that the law is generally exact in all the other planets: a single phænomena, or this same exactness not found, must not destroy this law, but must be looked upon as an exception, the particular reason of which we must seek after. In the second place, we might answer, as Mr. Cotes has done, that when even it should be allowed that the law of attraction is not exactly, in this case, in an inverted ratio of the square of the distance, and that this ratio is somewhat stronger, this difference may be estimated by calculation, and that we shall find it is almost insensible, since the ratio of the centripetal force of the moon, which must be the most troubled of all, approaches sixty times nearer the

ratio of the square than the ratio of the cube of the distance. "Responderi potest etiamsi concedamus "hunc motum tardissimum exinde prosectum quod "vis centripetæ proportio aberret aliquantulum a "duplicata, aberrationem illam per computum ma-"thematicum inveniri posse, & plane insensibilem esses; ista enim ratio vis centripetæ Lunaris quæ omnium maxime turbari debet, paululum quidem duplicatam superabit; ad hanc vero sexasiginta sere vicibus propius accedet quam ad triplisicatam. Sed verior erit responsio, &c.—Editoris
præf. in edit, 2. Mewton. Auctore Roger
Cotes."

In the third place, we must more positively anfwer, that this motion of the apfides does not proceed from the law of attraction's being fomewhat greater than the inverted ratio of the square of the distance; but because, in fact, the sun acts upon the moon by a force of attraction which must disturb its motion, and produce that of the apfides; and, consequently, that this might be the cause which prevents the moon from exactly following the rule of Kepler. Newton, in this light, has calculated the effects of this perturbative force; from his theory has drawn the equations and other motions of the moon with fuch a precision, that they very exactly answer, nearly to some seconds, to the observations of the best astronomers. But, to speak only of the motion of the apfides, he shews, in the 45th proposition of the first book, that the progresfion of the moon's apogee proceeds from the action of the fun; infomuch, that to this point all agrees, and his theory is found as true and as exact in all the most complicated cases as in those which are the leaft.

Nevertheless, Mr. Clairaut, one of our greatest geometricians, has pretended, that the absolute quantity of the motion of the apogee cannot be drawn

drawn from the theory of gravitation, fuch as it is established by Newton, because that by employing the laws of his theory, it is found, that this motion would not be finished in eighteen years, whereas it is completed in nine. spite of the authority of this able mathematician. and the reasons he has given to support his opinion, I have never been convinced, that Newton's theory agrees with his observations: I will not here undertake to make the examination which might be necessary to prove, that he is not fallen into the error which he is reproached with; I find. that it is shorter to ascertain the law of attraction. fuch as it is, and to shew that the law Mr. Clairaut would substitute in the room of that of Newton, is only a supposition which implies contradiction.

For admitting for a moment what Mr. Clairaut pretends to have demonstrated, that by the theory of mutual attraction the motion of the apfides would be made in eighteen years instead of nine; and let us remember, at the same time, that to the exception of this phænomena all the reft, however complicated they are, agree very exactly in this theory with obfervation: to judge thereon at first by probabilities, this theory must subsist, since there are a very considerable number of things where it perfectly agrees with Nature; that there is only a fingle case wherein it differs, and that it is very easy to be deceived in the enumeration of the causes of a fingle phænomena. It appears to me, therefore, that we might imagine one: for example, if the magnetic force of the earth can, as Newton fays, enter into the calculation, we should, perhaps, find that it influences the motion of the moon, and that it might produce this acceleration in the motion of the apogee; and it is in this case, or, in fact, we should employ two terms to express the measure of the forces which produce the motion of the moon: the first term of the expression expression would be always that of the law of univerfal attraction, that is to say, the inverted and exact ratio of the square of the distance; and the second term would represent the measure of the magnetical force.

This supposition is, without doubt, much better founded than that of Mr. Clairaut's, which feems to be much more hypothetical, and likewise subject to invincible difficulties. To express the law of attraction by two or more terms, add to the inverted ratio of the square of the distance a fraction of a squared fquare, instead of $\frac{1}{xx}$ place $\frac{1}{xx} + \frac{1}{m \times 4}$ appeared to me to be no more than to add an expression in such a manner as to correspond with it in every case. This is no longer a physical law which this expression reprefents; for by once suffering a second, a third, or a fourth term, &c. to be put, we might find an expression which in all the laws of attraction would represent the cases he works upon, by adjusting, at the same time, the motions of the apogee of the moon, and other phænomena; and, consequently, this supposition; if it was admitted, would not only annihilate the law of attraction in an inverted ratio of the square of the distance, but even would give entrance to all possible and imaginable laws: a law in physic is only a law because its measure is simple, and that the scale which represents it, is not only always the same, but also that it is singular, and cannot be represented by any other: now, every time that the fcale of a law shall not be represented by a fingle term, this simplicity and this unity of scale, which forms the effence of the law, no longer subfifts, and, consequently, there is no longer any physical law.

As this last reasoning might appear only metaphyfical, I shall endeavour to render it sensible by explaining myself more fully. I say, therefore, that every time that a law is wished to be established on the augmentation or diminution of a quality or phyfical quantity, we are strictly subjected to make use of only one term to express this law: this term is the representation of the measure which must vary, as, in fact, the quantity to be measured varies; so that if the quantity, at first only one inch, becomes afterwards a foot, an ell, a fathom, a mile, &c. the term which expresses it becomes expressively all these, or rather represents them in the same order of fize; and so it is likewise with every other ratio in

which a quantity can vary.

In whatfoever manner, therefore, we might fuppose a physical quality might vary; as this quality is one, its variation will be simple and always expresfible by a fingle term which will be the measure of it; and when we would make use of two terms, we deftroy the unity of the physical quality, because these two terms will represent two different variations in the fame quality; that is to fay, two qualities instead of one. Two terms are, in fact, two measures both unequally variable; and from thence they cannot be applied to a simple subject and a fingle quality. If we admit also two terms to express the effect of the central force of a planet, it is necessary to avow, that instead of one force there are two, one of which will be relative to the first term, and the other relative to the second term; from whence it is evidently feen, that it requires, in the present case, that M. Clairaut necessarily admits another force different from attraction, if he employs two terms to represent the total effect of the central force of a planet.

I know not how it can be imagined, that a physical law, such as attraction is, can be expressed by two terms by relation to the distances: for if, for example, there was a mass M, whose attractive virtue was expressed by $\frac{aa}{xx} + \frac{b}{x}$, would not there revolve.

fult the same effect as if this mass was composed of two different matters; as for example, of $\frac{1}{2}M$, whose law of attraction was expressed by $\frac{2aa}{xx}$, and of $\frac{1}{2}M$, whose attraction was $\frac{2b}{x4}$?—This appears to me absurb.

But, independent of these impossibilities, what does the supposition of M. Clairaut imply, which destroys also the unity of the law, on which the truth and beautiful simplicity of the system of the world is founded. This supposition is liable to so many other difficulties, that M. Clairaut ought methinks, to propose to himself before he admits it all the particular causes which might produce the same effect. I perceive, that if I had folved, as M. Clairaut, the problem of the three bodies, and had found that the theory of gravitation, in fact, gives only the half of the motion of the apogee, I should not have drawn the conclusion, that he draws against the law of attraction: this is also the conclusion I contradict, and to which I do not think any one obliged to subscribe, when even M. Clairaut should have been able to demonstrate the insufficiency of all the other particular causes.

Newton fays, (p. 547. vol. iii.) "In his computationibus attractionem magneticam terræ non
confideravi, cujus itaque quantitas perparva est
diano; ac ignoratur; si quando vero hæc attractio indiano, ac longitudines pendulorum isochronorum
in diversis parallelis, legesque motuum maris &
parallaxis Lunæ cum diametris apparentibus Solis
Lunæ ex phænomenis accuratius determinatæ
fuerint, licebit calculum hunc omnem accuratius
repetere." This passage does not very clearly
prove, that Newton has not pretended to have enumerated all the particular causes; and does he not,
in sact, indicate, that if we find some difference in

his theory and observations, that may proceed from the magnetical force of the earth, or from some other fecondary cause; and, consequently, if the motion of the apfides does not fo exactly agree with his theory as the rest, must we for that totally demolish his theory, by changing the general law of gravitation? or, rather, must we not attribute this difference, which is only found in this fingle phænomena, to other causes? M. Clairaut has proposed a difficulty against the system of Newton; but this is entirely only a difficulty which neither must nor can become a principle: we must strive to resolve it, and not to form a theory of it, all the consequences of which are only applied on a calculation: for, as I have obferved, we may represent every thing with a calculation, and not realize any thing: and if we place one or more terms successively, of the expression of a phyfical law, like that of attraction, it gives us no more than the arbitrary instead of representing us the reality.

On the whole, it is sufficient for me to have established the reasons which made me reject the supposition of M. Clairaut; and I think, that far from injuring the law of attraction, and overturning physical astronomy, it appears to me, on the contrary, to remain in its full vigour, and to have power to extend much farther, and that without my having pretended to have said near what might be said on this matter, to which it is my desire that every one would unprejudicedly give as much attention to judge of it

as it requires.

INTRODUCTION

TO THE

H I S T O R Y

OF

MINERALS,

EXPERIMENTAL PART.

THE following experiments contain many new matters, and others more ancient, fome of which have been printed, either in the Memoirs of the Academy of Sciences, or elsewhere, I have divided them into relative parts according to the different objects of the History of Nature, and have found many Memoirs which may be read independently one of the other, but which I have only connected according to the order of matters.

MEMOIR THE FIRST.

Experiments on the Progress of Heat in Fodies.

I caused ten bullets to be made of forged and beaten iron: the first, of half-inch diameter; the second, of an inch; the third, of an inch and an half, and so on progressively to sive inches. This iron came from the forge of Chamæecon, near Chatillon-sur Seine: and as all the bullets were made of iron of the same forge, their weights are found nearly proportionable to their volumes.

The

The bullet of half an inch weighed 190 grains, or 2 drams, 46 grains, Paris weight; that of an inch, 1522 grains, or 2 ounces, 5 drams, 10 grains; that of an inch and an half, 5136 grains; that of two inches, 12173 grains; that of two inches and an half, 23781 grains; that of three inches, 41085 grains; that of three inches, and an half, 65254 grains; that of four inches, 97388 grains; that of four inches and an half, 138179 grains; that of five inches, 190211 grains.

All these weights were justly taken with very good scales, by adjusting by degrees those bullets which

were found a little too heavy.

Before I relate the experiment, I shall observe, first, that while they were making, the thermometer, exposed to the open air, was at the freezing point, or some degrees above; but that the bullets were suffered to cool in a pit where the thermometer was nearly ten degrees below that point; that is to say, to the degree of temperature of the pits of the observatory, and it is this degree which I have here taken for that of the actual temperature of the earth.

Secondly, I have endeavoured to take two different methods in the cooling; the first where they ceased to burn, that is to say, when they might be held in the hard hand a second without burning: the second were as cold as the actual temperature. To know the exact moment of this cooling to the actual temperature, other bullets were made use of for comparison of the same matters, and of the same diameters which were not heated, and which were selt at the same time as the others. By this immediate and simultaneous touch of the hand, or two hands, on the two bullets, we could well enough judge of the moment when these bullets were equally cold: this simple manner is not only easier than the thermometer which was with difficulty applied here; but it

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to judge of the equality and not of the proportion of the heat, and that our senses are the best judges of what is absolutely equal or perfectly similar. On the whole, it is easier to distinguish the moment when the bullets cease from burning, than that when they are cooled to the actual temperature, because that a live sensation is always more precise than a temperate sensation, as the first affects us in a stronger manner.

Thirdly, As the more or less smoothness or roughness on bodies makes much difference to the touch, and as a smooth body seems to be colder if it is cold, and hotter if it is hot, than a rough body of the same matter, although they are both equally so, I have taken care that the cold bullets were rough, and like those which had been heated, whose surfaces were sprinkled over with little

eminences produced by the fire.

EXPERIMENTS.

T.

The bullet of $\frac{1}{2}$ inch was heated white in two minutes, cooled so as to be held in the hand in twelve, and to the actual temperature in thirty-nine minutes.

II.

The bullet of an inch, heated white in five minutes and an half, cooled so as to be held in the hand in thirty-five minutes and an half, and to the actual temperature in one hour and thirty-three minutes.

III.

The bullet of an inch and an half, heated white in nine minutes, cooled so as to be held in the hand in thirty-five minutes, and to the actual temperature in two hours twenty-five minutes.

IV.

The bullet of two inches heated white in thirteen minutes, cooled so as to be held in the hand in one hour twenty minutes, to the actual temperature in three hours fixteen minutes.

V.

The bullet of two inches and an half heated white in fixteen minutes, cooled so as to be held in the hand in one hour forty-two minutes, to the actual temperature in four hours thirty minutes.

VI.

The bullet of three inches heated white in nineteen minutes and an half, cooled so as to be held in the hand in two hours seven minutes, to the actual temperature in five hours, eight minutes.

VII.

The bullet of three inches and an half heated white in twenty-three minutes and an half, cooled so as to be held in the hand in two hours thirty-fix minutes, to the actual temperature in five hours, fifty-fix minutes.

VIII.

The bullet of four inches heated white in twentyfeven minutes and an half, cooled so as to be held in the hand in three hours two minutes, to the actual temperature in fix hours fifty-five minutes.

The bullet of four inches and an half heated white in thirty-one minutes, cooled in three hours twenty-five minutes, to the actual temperature in feven hours forty-fix minutes.

The bullet of five inches heated white in thirtyfour minutes, cooled in three hours fifty-two minutes, to the actual temperature in eight hours forty-two minutes.

The

The most constant difference that can be taken be tween each of the terms which express the time of cooling, from the instant the bullets were drawn from the fire, to that when we can touch them unthurt, is found to be about twenty-four minutes; for, by supposing each term to increase twenty-four, we shall have 12, 36, 60, 84, 108, 132, 156, 180, 204, 228 minutes. And the the continuation of the real times of these coolings, are 12, 35½, 58, 80, 102, 127, 156, 182, 205, 232 minutes, which approaches the first as nearly as experiment can approach calculation.

So likewise the most constant difference to be found between each of the terms of cooling to actual temperature, is found to be fifty-four minutes; for by supposing each term to increase fifty-four, we shall have 39, 93, 147, 201, 255, 309, 363, 417, 471, 525 minutes: and the continuation of the real time of this cooling, is found by the preceding experiments to be 39, 93, 145, 196, 248, 308, 356, 415, 466, 522 minutes, which approaches also

nearly to the first.

I have made the like experiments upon the same bullets twice or thrice; but I have found I could only rely on the first, because I perceived, that each time the bullets were heated, they lost considerable weight: but all this great loss of weight is not only occasioned by the falling off of the parts of the surface reduced into seoria, but also by a kind of drying or internal calcination which diminishes the weight of the constituent parts of the iron; insomuch, that it appears that a strong sire renders the iron specifically lighter each time it is heated. On the whole, I have found by ulterior experiments, that this diminution of weight varies much, according to the different quality of the iron.

Having, therefore, caused fix new bullets to be made from half an inch to three inches diameter,

and of the same weight as the first, I have found the same progression as well in the ingress as in the egress of the heat; and I am certain that iron heats and cools, in fact, as I have explained. A passage of Newton's gave birth to these experiments.

"Globus ferri candentis, digitum unum latus, calorem suum omnem spatio hora unius in aere consistens, vix amitteret. Globus autem major calorem diutius conservaret in ratione diametri, propterea quod superficies (ad cujus mensuram per contactum aeris ambientis refrigeratur) in illa ratione minor est pro quantitate materia sua calida inclusa. Ideoque globus ferri candentis huic terra aqualis, id est, pedes plus minus 40000000 latus, diebus totidem & ideirco annis 50000, vix refrigesceret. Suspicor tamen quod duratio caloris ob causas latentes augeatur in minori ratione quam ea diametri; & optarim ratio-

" nem veram per experimenta investigari."

Newton, therefore, defired that the experiments I have laid down might be made, and I am determined to attempt them, not only because I required them for views similar to his, but also because that I have thought this great man might be deceived, by saying, that the duration of heat must not increase by the effect of hidden causes, but in a less ratio than its diameter. It has appeared to me, on the contrary, by reslection, that these hidden causes might only render this ratio greater instead of making it less.

It is certain, as Newton fays, that a larger globe will preserve its heat longer than a smaller by reason of the diameter, if these globes are supposed to be composed of a matter perfectly permeable to heat; so that the egress of the heat was perfectly free, and the igneous particles should not meet with any obstacle to stop or change their direction: it is only in this mathematical supposition, that the duration of Vol. V.

the heat would be, in fact, in a ratio of the diameter. But the hidden causes which Newton speaks of, and the principal of which are the obstacles which refult from the unabsolute, imperfect, and unequal permeability of all folid matter, instead of diminishing the time of the duration of heat, must, on the contrary, increase it. This has appeared to me fo clear, even before I had attempted my experiments, that I should be inclined to think, that Newton, who faw fo clear even in things themfelves, which he only suspected, is not fallen into this error; and that the word minori ratione, instead of majori, is only a fault of his pen, or his copyist, which are scattered about in the editions of his work, at least, in all those I have consulted: my conjecture is so much the better founded, as Newton appears to fay elsewhere precisely the contrary. In the 11th question of his Essay on Optics, he says, "Bodies of a great volume, do not they longer " preserve (Note. This word LONGER, can here only " fignify in a greater ratio than that of the diameter.) " their heat, because their parts heat reciprocally? "And a vast, dense, and fixed body being once heat-" ed beyond a certain degree, can it not throw " light in fuch abundance, as by the emission and " re-action of its light, by the reflexions and re-" fractions of its rays within its pores, it becomes " always hotter till it attains a certain degree of " heat which equals the heat of the fun? And the " fun, and the fixed stars, are they not vast earths " violently heated, where heat is preferved by the " largeness of these bodies, and by the reciprocal " action and re-action between them and the light " they emit, their parts being besides prevented " from evaporating in fmoke, not only by their " fixity, but also by the vast weight and the great " denfity of the atmospheres which compress them very strongly on all fides, and increase the va-" pours

pours and exhalations which rise from these bo

By this passage we see, that Newton is not only here of my opinion, on the duration of heat which he supposes in a greater ratio than that of the diameter, but also that he advances too much on this augmentation, by saying, that a greater body, because it is great, can augment its heat.

Be it as it will, experience has fully confirmed my opinion: the duration of heat, or the time taken up in the cooling of iron, is not in a smaller, but in a larger ratio than that of the diameter: to be affured of it, we need only compare the following progreffions.

DIAMETERS.

1, 2, 3, 4, 5, 6, 7, 8, 9, 10 half inches.

The time of the first refrigeration, supposed in ratio of the diameter, 12, 24, 36, 48, 60, 72, 84, 96, 108, 120 minutes.

The real time of this refrigeration, discovered by experience, 12, $35\frac{1}{2}$, 58, 80, 102, 127, 156, 182, 205, 232 minutes.

The time of the second refrigeration, supposed in ratio of the diameter 39, 78, 117, 156, 195, 235, 273, 312, 351, 390 minutes.

The real time of this second refrigeration, discovered by experience, 39, 93, 145, 196, 248, 308,

356, 415, 466, 522 minutes.

By comparing these progressions term to term, we find, that in every case the duration of heat is not only in a smaller ratio than that of the diameter, (as Newton has written) but, on the contrary, is in a considerable larger ratio

Confiderable larger ratio.

Dr. Martin, who has composed a good work on thermometers, relates this passage of Newton, and says, that he had begun to make some experiments, which he proposed to carry on farther: that he thought the opinion of Newton to be conforma-

ble

ble to truth, and that fimilar bodies, in fact, retain their heat in proportion of their diameters; but that if this proportion in great bodies is not less than that of the diameters, he thinks it not fufficiently founded. Dr. Martin had reason in this respect; but, at the same time, he was wrong in thinking, after Newton, that all fimilar folid or fluid bodies retain their heat by reason of their diameters. He truly relates experiments made with water in china basons; by which he found, that the times of the refrigeration of the water are almost proportionable to the diameters of the basons which contain it. But we have just feen, that it is even for this reason, that the matter acts differently in folid bodies; for water must be looked upon as a matter almost entirely permeable to heat, fince it is an homogenous fluid, and that none of its parts can make any obstacle to the circulation of the heat. Thus, although the experiments of Dr. Martin nearly affords the ratio of the diameter for the refrigeration of water, we must not draw any conclusion therefrom for the refrigeration of folid bodies.

Now, if we would with Newton, learn how long time it would require for a globe as large as the earth to cool, we should find, after the preceding experiments, that instead of 50000 years, which he assigns for the time for the earth to cool to the actual temperature, it would require 42964 years, 221 days, to cool it only to the point where it should cease to burn, and 86667 years and 132 days to cool

it to the actual temperature.

For the diameters of the globes being 1, 2, 3, 4, $5\frac{1}{2}$... N. inches, the time of refrigeration till they might be touched without burning, will be 12, 36, 60, 84, 108-24 N. -12 min. and the diameter of the earth, being 2865 miles, of 25 to the degree, or 6537930 fathom of 6 feet.

By

By making the mile 2282 fathom, or 39227580 feet, or9414619201 inches, we have N=9414619201 inches, and 24 N-12=22598086068 min. that is to fay, 42964 years and 221 days for the time neceffary for a globe like the earth to cool only to the

point of not burning.

So also the times of refrigeration to the actual temperature, will be 39, 93, 147, 201, 255... 54 N-15 min. and as N is always = 941461920 1 inches, we shall have 54 N-15=50838943662 min. that is to fay, 86670 years and 132 days, for the time necessary to refrigerate a globe as large as the

earth to the actual temperature.

It might only be supposed, that the refrigeration of the earth should be still considerably increased, because we imagine that refrigeration is only performed by the contact of the air, and that there is a great difference between the time of refrigeration in the air, and the time of refrigeration in vacuo; and as it is to be supposed, that the earth and air cools in the fame time in vacuo, this furplus of time should be reckoned. But it is easy to shew that this difference of time is very inconfiderable; for, though the denfity of the medium in which a body cools, makes fomething on the duration of the refrigeration, this effect is much less than might be imagined; fince in mercury, which is ten thousand times denfer than air, to refrigerate bodies, it is only requifite to plunge them therein about nine times as often as is required to produce the fame refrigeration in air.

The principal cause of refrigeration is not, therefore, the contact of the ambient medium, but the expansive force which animates the parts of heat and fire, which drives them out of the bodies wherein they refide, and impels them directly from the cen-

ter to the circumference.

In the preceding experiments, by comparing the time employed to heat iron globes, with the time requifite to cool them, we shall find, that about the fixth part and an half the time is necessary to heat them till they become white, to what is necessary to cool them to the point that they may be held in the hand, and about the fifteenth and an half part to cool them to actual temperature; fo that there is still a very great correction to be made in Newton's text, in the estimate he has made on the heat communicated by the fun to the comet of 1680: for this comet, not having been exposed to the violent. heat of the fun but a short time, it could not receive it only in proportion of this time, and not wholly as Newton supposes it in the following pasfage.

"Est calor Solis ut radiorum densitas, hoc est re-" ciproce ut quadratum distantiæ locorum a Sole. " Ideoque cum distantia cometæ a centro Solis decemb. 8, ubi in perihelio versabatus, esset ad " diffantiam terræ a centro Solis ut 6 ad 1000 cir-" citer, calor Solis apud cometam eo tempore erat " ad calorem Solis æftivi apud nos ut 1000000 ad 6 36, feu 28000 ad 1. Sed calor aquæ ebullientis eft " quasi triplo major quam calor quem terra arida " concipit ad æstivum Solem ut expertus sum, &c. " Calor ferri candentis (fi recte conjector) quali tri-" plo vel quadruplo major quam calor aquæ ebulli-" entis; ideoque calor quem terra arida apud Co-" metam in perihelio versantem ex radiis solaaibus " concipere posset, quasi 2000 vicibus major quam calor ferri candentis. Tanto autem calore vapores " & exhalationes, omnisque materia volatilis statim

"Cometa igitur in perihelio suo calorem immenfum ad Solem concepit & calorem illum diutissime conservare potest."

" confumi ac diffiari debuiffent.

I shall

I shall first remark, that Newton here makes the heat of red-hot iron much less than it, in fact is; and he himself says, in a Memoir, entitled The Scale of Heat, and which he has published in the Philofophical Transactions of 1701; that is to say, many years before the publication of his Principles. fee in this excellent Memoir, which includes the germ of all the ideas on which thermometers have afterwards been constructed; we there find, I say, that Newton, after very exact experiments, makes the heat of boiling water to be three times greater than that of the fun in the height of fummer; that of melted tin, fix times greater; that of melted lead, eight times; that of melted regulus; twelve times; and that of a common culinary fire, fixteen or feventeen times greater than that of the fummer's fun: and from hence, we cannot help concluding, that the heat of red-hot iron to white, is still much greater, fince it requires a fire continually animated by the bellows to heat the iron to this degree: Newton feems to be fenfible of it himself, and gives us to understand, that this heat of red-hot iron appears to be feven or eight times greater than that of boiling water. This diminishes half the heat of this comet. compared to that of hot iron.

But this diminution, which is only relative, is nothing in itself, nor in comparison of the real and very great diminution which results from our first consideration. For the comet to have received this heat a thousand times greater than that of red-hot iron, it would require to have remained for a very long time in the vicinity of the sun; whereas, it has only passed it very rapidly at a small distance, on which alone, nevertheless, Newton established his Comparative Calculation. It was on the 8th of December, 1680, at \(\frac{6}{1000}\) distance from the earth to the center of the sun; but twenty-sour hours before,

and as many after, it was at a distance fix times greater, and where the heat was consequently thirty-fix times less.

If, then, we would know the quantity of this heat communicated to the comet by the fun, we here find how we should make this estimation tolerably just; and, at the same time, make the comparison with hot iron by the means of my experiments.

We shall suppose as a fact, that this comet has taken up 666 hours to descend from the point where it was then distant from the sun at an equal distance. as the earth is from that planet, at which point the comet confequently received an heat equal to that which the earth receives from the fun, and which I here take for unity: we shall likewise suppose, that the comet has taken up 666 hours more to ascend from the lowest point of its perihelium to this same distance; and supposing also its motion uniform, we shall perceive, that the comet, being at the lowest point of its perihelium; that is to fay, to $\frac{6}{1000}$ of the distance from the earth to the sun: the heat it received in that moment was 2700766 times greater than that the earth receives. By giving to this moment a duration of 80 minutes; to wit, 40 minutes for its descent, and 40 for its ascent, we shall have at 6 diffance, 27776 heat during 8 minutes; at 7 distance, 20408 heat also during 80 minutes; at 8 distance, 15625 heat during 80 minutes; and thus, successively, to the distance 1000, where the heat is 1. By fumming up all the heats at each distance, we shall find 363410 to be the total of the heat the comet has received from the fun, as much in descending as in ascending, which must be multiplied by the time; that is to fay, by four thirds of of an hour, we shall then have 484547, which we shall divide by 2000, which represents the folid heat the earth has received in this time of 1332 hours, fince the distance is always 1000, and the heat always equals = 1. Thus, we shall have $242\frac{547}{2000}$ for the heat the comet has received more than the earth during the whole time of its perihelium, instead of 28000, as Newton supposes it, because he takes only the extreme point, and pays no attention to the very small duration of time.

And still this heat must be diminished 242 547 because the comet run, by its acceleration, as much more way in the same time, as it was nearer the sun.

But by neglecting this diminution, and by admitting only that the comet, in fact, received an heat nearly 242 times greater than that of our fummer's fun; and, consequently, 17% times greater than that of hot iron, according to Newton's estimation; or only ten times greater, according to the correction of this estimation. It must be supposed, that to give an heat ten times greater than that of red-hot iron, it required ten times more time; that is to say, 1332; consequently, we may compare the comet to a globe of iron heated by a forge sire for 13320 hours, to heat it to a whiteness.

Now we find by my experiments, that the time necessary to heat globes, whose diameters increase; as 1, 2, 3, 4, 5 · · · · $N\frac{1}{2}$ inches, is very near 2; $5\frac{1}{2}$, g, $12\frac{1}{2}$; $16 \cdot \cdot \cdot \cdot \cdot \frac{7^{n-3}}{2}$ min. we shall then have $\frac{7^{n-3}}{2}$ =799200 minutes; from whence we shall draw

n=2283421 inches.

Thus, with a forge fire, we can heat to a whiteness a globe whose diameter is 228342½ inches, only
in 799200 minutes, or 13320 terms, and, consequently, it would require for the whole mass of the
comet to be heated to the point of iron to a whiteness, during the short time it was exposed to the
heat of the sun, that it had only 223342½ inches
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diameter; and to suppose also, that it had been flruck on all fides, and at the fame time, by the light of the fun: from hence it refults, that if we suppose it greater, we wult necessarily suppose more time in the same ratio of n=0414619201 inches. and 21-3=3295116718 minutes; i.e. that instead of a year of 190 days, it would require the comet, instead of having only remained 1332 hours, or 35 days 12 hours in all its perihelium, had remained there 392 years. Thus comets, when they approach the fun, do not receive an immense heat, nor a very durable one, as Newton fays, and as we might be inclined to believe at the first view. Their flay is so short in the vicinity of this planet, that these masses have not time to be heated, and that there is scarcely only part of the surface exposed to the fun, which is burned by these instances of extreme heat, which, by calcining and volatilizing the matter of this furface, drives it outwardly in vapours and in dust from the opposite side to the sun; and what is called the tail of the comet, is nothing elfe than the light even of the fun rendered fenfible, as in a dark room, by those atoms which the heat lengthens as it is more violent.

But another very different and still more important consideration is, that to apply the result of our experiments and calculation to the comet and earth, we must suppose them composed of matters which would demand as much time as iron to cool; whereas, in reality, the principal matters of which the terrestrial globe is composed, such as clay, gres, stones, &c. must cool in a much less time than

iron.

To fatisfy myself on this point, I caused globes of clay and marl to be made; and having heated them at the same forge to a whiteness, I found that the clay balls of two inches, cooled in thirty-eight minutes

minutes fo as to he held in the hand; those of two inches and an half, in forty-eight minutes; and those of three inches, in fixty minutes; which, being compared with the time of the refrigeration of iron bullets of the same diameters of two inches, two inches and an half, and three inches, give 38 to 80 for two inches, 48 to 102 for two inches and an half, and 60 to 127 for three inches, which makes a little less from one to two, so that for the refrigeration of clay, only half the time is required than is for iron.

I have found also, that lumps of clay or marl of two inches, refrigerated so as to hold them in the hand in forty-five minutes; those of two inches and an half, in fifty-eight; and those of three inches in seventy-five, which, being compared with the time of refrigeration of iron bullets of the same diameters, gives 46 to 80 for two inches, 58 to 102 for two inches and an half, and 75 to 127 for three inches, which nearly form the ratio of 9 to 5; so that for the refrigeration of clay, more than half the time is re-

quired than for iron.

I shall observe on the subject of these experiments, that globes of clay heated white, loft more of their weight than iron bullets, even to the ninth or tenth part of their weight; whereas gres, heated in the fame fire, loses scarcely any thing at all of its weight, although the whole furface is covered over with scales, and reduced into glass. As this little matter appeared fingular, I repeated the experiments feveral times, increasing the fire, and continuing it longer than for iron; and although it fcarcely required a third of the time to redden clay, than what it did to redden iron, I kept them in this fire double and treble the time, to fee if it would lofe more, and I found only very: trifling diminutions: for the globe of two inches, heated for eight minutes, which weighed feven ounces two drams thirty grains before it was put in the fire, loft only forty-one grains, which does not make an hundredth hundredth part of its weight; and that of three inches, which weighed twenty four ounces, five drams, thirteen grains, having been heated by the fire for eighteen minutes, that is to fay, nearly as much as iron, lost only to feventy-eight grains, which does not make the hundred and eighty-first part of its weight. These losses are so trisling, that they may be looked upon, in fact, as no losses; and, in general, as certain that pure clay loses nothing at all of its weight in the fire: for it appears, that those trisling diminutions have been occasioned by the ferruginous parts which are found in this clay, and which

have been in part destroyed by the fire,

One thing more general, and which deferves notice is, that the duration of heat in different matters exposed to the same fire for an equal time, are always in the same proportion, whether the degree of heat is greater or smaller; so that, for example, if iron, clay, and gres is heated by a violent fire, and such as require eighty minutes to refrigerate iron so as to be held in the hand, forty-fix minutes to refrigerate gres to the same point, and thirty-eight to refrigerate clay and that at a less heat it would require, for example, only eighteen minutes to refrigerate iron to this same degree, it would require proportionally only a little more than ten minutes to refrigerate gres, and about eight minutes and an half to refrigerate clay to the same degree.

I have made the like experiments on marble, flone, lead, and tin, by a heat only strong enough to melt tin, and I found, that iron refrigerated in eighteen minutes, marble refrigerated to the same degree in twelve minutes, stone in eleven, lead in nine, and

tin in eight.

It is not, therefore, proportionally to their denfity, as is vulgarly supposed, that bodies receive more or less heat, but in a quite different relation, which is in an inverted ratio of their solidity; that is to say,

of their greater or lesser non fluidity; so that, by the same heat, less time is requisite to heat or cool the most dense sluid. In the following Memoirs I shall give the entire development of this principle, on which all the theory of the progress of heats depends; but that my affertion should not appear vain, here follows in a few words the foundation of this theory.

I have found that bodies which should heat in ratio of their diameters, could be only those which might be perfectly permeable to heat, and that might be, at the same time, such as would heat or cool in the same time. From hence, I have thought that sluids, whose parts are only held together by a slight connection, might approach nearer this perfect permeability than solids, whose parts have much more cohetion than those of sluids.

In consequence of this, I made experiments, by which I have found, that with the same heat all fluids, however dense soever they be, heat and cool more readily than any folid, however light it may be; so that, for example, mercury, compared with wood, heats much more readily than wood, although it be fifteen or fixteen times denser.

This made me perceive, that the progress of heat in bodies must not in any case be made relatively to their density; and, in fact, I have found by experience, that this progress, as well in solids as sluids, is made rather by reason of their fluidity, or in an inverted ratio of their solidity.

As the word Solidity has many acceptations, we must clearly see the sense in which I here make use of it. Solid and solidity are spoken of in geometry, relatively to fize, and are received for the volume of a body. S lidity is often spoken of in physics, relatively to density, that is to say, to the mass contained under a given volume. Solidity is often spoken of also relatively to hardness; that is to say, to the resistance that bodies make when we would impel them. Now

it is in none of these meanings that I make use of this word, but in an acceptation which ought to be the first, because it is the most proper. I understand solely by solidity the quality opposite to fluidity; and I say, that it is in an inverted ratio of this quality that the progress of heat is made in most bodies, and that they heat or cool so much the safter as they are the more sluid, and so much the slower as they are more

folid, every other circumstance being equal.

To prove also, that solidity, taken in this sense, is perfectly independent of density, I have sound by experience, that the most or least dense matters, heat or cool more readily than other more or less dense matters: that, for example, gold or lead, which are much more dense than iron and copper, nevertheless heat and cool much quicker; and that tin and marble, which are, on the contrary, not so dense, heat and cool much faster than iron and copper; and that there is likewise many other matters, which, although more or less dense, heat and cool more readily than others which are much less or more dense; so that density is no ways relative to the scale of the progress of heat in solid bodies.

To prove it likewife in fluids, I observed, that quickfilver, which is thirteen or fourteen times denfer than water, nevertheless heats and cools in less time than water; and that spirit of wine, which is less dense than water, heats and cools also quicker than water; fo that generally the progress of heat in bodies, as well for the ingress as egress, has an affinity with their denfity, and is principally made in ratio of their fluidity, by extending the fluidity to a folid, that is to fay, by looking on folidity as greater or a leffer non fluidity. From hence I have thought, I ought to conclude, that we should know, in fact, the real degree of fluidity in bodies, by heating them to the same heat; for their sluidity would be in a like ratio as that of the time during which they will receive

ceive and lose this heat: and it will be the same with solid bodies. They will be so much the more solid, that is to say, so much the more non fluids, as they will require more time to receive and lose this heat, and that almost generally to what I presume: for I have already tried these experiments on a great number of different matters, and I made a table, which I have endeavoured to render as complete and exact as possible, and which will be found in the sollowing Memoir.

SECOND MEMOIR.

Continuation of the Experiments on the progress of Heat in different Mineral Substances.

I caused a great number of globes to be made of an inch diameter, the most precise as was possible, from the following matter, which may represent nearly the mineral kingdom.

Now the present gold refined by M. Tillet, of the Academy of Sciences, who made this globe at my

number of the forms which were too

request,

Weighed -			gt. 17	Emerald - 1 2 244
Lead		1	28	Marble white 1 0 25
Pure filver -	3	3	22	Pure clay - 0 7 24
Bismulk	3	0	3	Marble com-
Copper red -	2	7	56	monof Mont-
Ison			10	bard - 0 7 20
Tin 10-		3	48	Hard and grey
Antimony melt-			C fut	calcar. stone
ed and which		Lo	is tall	of Montbard o 7 20
had fmall ca-		The second	11 70	White gypfum,
vities on its				improperly
furface	2	1	34	called Ala-
Fine -	2	Į	2	bafter - 0 6 36
				Calca-

oz. d. gra	oz. d. grs
Calcar : ous white	Pure earth, very
ftone, of the	dry - 0.6 16
quarry of A-	Oker 0 5 9
nieres, near	Porcelain of the
Dijon. 0 6 36	court de Lau-
Rock Crystal;	raguais = 0 5 21
it was a little	White chalk 0 4 49
too fmall, and	Cherrywood
had many de-	which altho
fects. I pre-	lighter than
fume that	most other
without them	woods, is that
it would have	which takes
weighed 0 6 22	in the least
Common glass o 6 21	fire - 0 1 59

I must here observe, that we must not rely on the weight set down in this table, to conclude therefrom the exact specifical weight of each matter; for whatsoever precaution I took to render the globes equal, as I was obliged to employ different workmen, some were too large and others too small. Those which were more than an inch diamer were diminished, but some which were too small, as those of rock crystal glass and porcelain, remained as they were. I have only rejected those of agate, jasper, and pophyry, which were sensibly too small. Nevertheless, this degree of precision in size, was not absolutely necessary, for it could very little alter the result of my experiments.

Before I had ordered all these globes of an inch diameter, I exposed to a like degree of fire, a square mass of iron and another of lead of two inches, and found by reiterated essays, that lead heated and cooled quicker in much less time than iron. I made the same heat on red copper, it required also more time to heat and cool than lead

and

and less than iron. So that of these three matters, ifon appeared the least accessible to heat, and, at the fame time, that which retained it the longest. This made me know that the law of the progress of heat, that is to fay, of its ingress and egress in bodies, was not at all proportionable to their denfity, fince lead. which is denfer than iron and copper, nevertheless heats and cools in less time than these two other metals. As this object appeared important, I caused my little globe to be made, to be more perfectly fatisfied on a great number of different matters of the progress of heat in each. I always placed the globes at an inch distance from each other, before the same fire, or in the fame oven, 2, 3, 4 or 4, &c. together during the fame time with a globe of tin in the midst of the reft. In most of my experiments, I suffered them to be exposed to the same active fire, till the globe of tin began to melt, and at that instant they were all removed together, and placed on a table in fmall cases. I suffered them to cool without moving them, by often trying whether I could touch them and the moment they left off burning the fingers, and I could hold them in my hands half a fecond, I marked the number of minutes which were paffed fince I drew them from the fire. I afterwards fuffered them to cool to the actual temperature, of which I endeavoured to judge by means of other small globes of the fame matter which had not been heated. and which I touched at the fame time as the others' cooled. Of all the matters which I put to the trial. there is only fulphur which melts in a less degree of heat than tin, and in spite of its disagreeable finels I should have taken it for a term of comparifon, but as it is a brittle matter which dimimishes by friction, I preferred tin, although it require ed nearly double the heat to melt it, than fulphur does.

than aron. So that of thefe three matters

By the first experiment, the leaden and copper bullet heated in the same time, and cooled in the following order:

Cooled fo as to be keld in the	Cooled to actual temperature.
Cooled fo as to be held in the hand for half a second.	on made denier chart in the Min
Lead in 870 Copper 12	In 23
Copper 12	In

Having heated the bullets of iron, copper, lead, tin, gres and montbard marble together in the fame fire, they cooled in the following order:

Cooled so as to be held in the hand for half a second.	Cooled to actual temperature.
Tin in 64	In 2 2 2 16
Lead in 8 Gres in 9	In _ 17
Commom marble in 10 Copper in 111	In - 21 In - 30 In - 38
Iron in 13	1 in 3*

TT.

By a fecond experiment by a fiercer fire, sufficient to melt the tin bullet, the five other bullets cooled in the following proportions:

Gooled fo as to be held in the hand for balf a fecond.	Cooled to the temperature,
Min.	Mint
Lead in 104	in - 42
Gres in 121	In 1- 1- 1- 1- 1- 1- 1- 1- 1- 1- 1- 1-
Common marble - 131	In - 50
Copper 194	In - 51
Iron 231	In 54
- A Company of the Company of the Company	Ву

III.

By a third experiment, by a less degree of fire than the preceding, the same bullets, with a fresh tin bullet, cooled in the following order:

Cooled so as to be held in the band balf a second.	Cooled to temperature.
Tin in 71	In 25
Lead in 91 Gres in 101	In the same with a course
Common marble in 12	In 37
Copper in 14	In 44 In 50
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From these experiments, which I have made with as much precision as possible, we may conclude, first, that the time of refrigeration of iron, so as to be held in the hand, is to that of copper::531:45, and to the point of temperature::142:125.

2dly, That the time of refrigeration of iron, so as to be held in the hand, is to that of the first refrigeration of common marble: : 53½:35½, and their entire refrigeration:: 142:110.

3dly, That the time of refrigeration of iron, to that of gres, so as to be held in the hand, is:: 53½: 32 and :: 142: 1021, for their entire refrigeration.

4thly, That the time of refrigeration of iron to that of lead, so as to be held in the hand, is:: 53½: 27 and:: 142:94½ for their entire refrigeration.

int

46

50

51

54 By

V

As there were but two experiments for the comparison of iron with tin, I made a third, in which the tin cooled so as to be held in the hand in eight minutes, and entirely to the temperature in thirty-two; and the iron cooled to be held in the hand in eighteen, and entirely cooled in forty-eight, by means of which the proportion of the three experiments, is, first, for the refrigeration of iron compared with tin: 48:22,

and:: 136:73 for their entire refrigeration. Sea condly, that the time of the refrigeration of copper is to that of common marble:: 45:35! for the entire refrigeration, and:: 125: 102 for the refrigeration to the temperature. Thirdly, that the times of refrigeration of copper are to that of gres:; 55:33 for the first refrigeration, and::125:102 for the refrigeration to the actual temperature. Fourthly, that the times of refrigeration of copper are to those of refrigeration of lead::45:27 for the first refrigeration, and::125:94\frac{1}{2} for the whole,

VI.

As there were for the comparison of copper and tin only two experiments, I made a third, in which the copper cooled to be held in the hand in eighteen minutes, and entirely in forty-nine. Tin cooled to the first point in eight minutes and an half, and to the halt in thirty; from whence it may be concluded, first, that the time of refrigeration, so as to be held in the hand, of copper is to that of tin: : 421: 221, and :: 123:71 for their entire refrigeration. Secondly, it may likewife be concluded, from the preceding experiments, that the time of refrigeration of common marble is to that of the refrigeration of gres, fo as to be held in the hand, :: 361 : 32, and :: 110: 102 for their entire refrigeration. that the time of refrigeration of marble is to that of lead, fo as to be held in the hand, :: 361: 28, and :: 110: 941 for the entire refrigeration.

- En puer off of the consider VIL.

As there were only two experiments for the comparison of marble with tin, I made a third, in which the tin cooled so as to be held in the hand in nine minutes, and the marble in eleven; and the tin cooled entirely in twenty-two minutes and an half, and the marble in twenty-three; thus the times of refrigeration of marble, are to those of tin, as 33 is to 241 for the first refrigeration, and :: 93: 64 for the fe-

VIII.

As there were only two experiments for the comparison of gres and lead with tin, I made a third by heating together these three bullets of gres, lead, and tin, which cooled in the following order:

Cooled so as to be held in the	Cooled to the temperature.
hand. Min.	
Tin in 71	
Lead in 8½ Gres in 10½	

Thus it may be concluded, first, that the time of refrigeration of lead is to that of tin, so as to be held in the hand: $25\frac{1}{2}$: $21\frac{1}{2}$, and: $79\frac{1}{2}$: 64 for their entire refrigeration. Secondly, that the time of refrigeration of gres is to that of tin, so as to be held in the hand: $70:21\frac{1}{2}$, and: 84:64 for their entire refrigeration. Thirdly, so likewise it may be concluded, by the four preceding experiments, that the time of refrigeration of gres is to that of lead, so as to be held in the hand: $42\frac{1}{2}:35\frac{1}{2}$, and: $121\frac{1}{2}$ for their entire refrigeration.

IX.

In an oven hot enough to melt tin, although all the coals and cinders were drawn out, I placed on a piece of iron wire, five bullets, distant from one another about nine lines, after which the oven was thut, and having drawn them out about eighteen minutes, they cooled in the following order:

Cooled so as to be beld in the	Cooled to the temperature.
hand half a second.	remaid by the preceding
Melted tin in - 8	
Silver in 1 - 14	
18700	Gold

Gold in 1 48 1- 15	Init of endanti-la 46
Copper in 164 Iron in 18	Intoine die hil-onigo
Iron in - 18	In 56.

And permit have been to West that prove and the

In the fame oven, but with a flower heat, the fame bullets, with another bullet of tin, cooled

it demonstrated but belong well a this he

So as to be held in the hand	Cooled to the temperature.
balf a second. Min,	Min.
Silver in 11	
Gold in 124 Copper in 14	In + 40
Iron in	[4] Z (1) [2] [3] [4] [4] [4] [4] [4] [4] [4] [4] [4] [4

C : XI.

In the same oven, but with a still less degree of heat, the same bullets cooled in the following proportions:

Gooled fo as to be beld in the hand.	Cooled	to the	tempe	rature.
Tin in 6	In			Min. - 17 - 26
Silver in 9 Gold in 9	In		•	- 28
Copper in - 10 Iron in 7 - 11	In In	7	onio e	- 35

From these experiments it may be concluded,

1. That the time of the refrigeration of iron is to
that of copper, so as to be held in the hand,

1: 11+16+18: 10+14+164, or :: 454: 404 by
the three present experiments: as this also has been
found by the preceding experiments (Article IV.)

1: 534: 45, we shall have, by adding these times
190 to 852, for the still more precise relation of the
these trefrigeration of iron and copper; and for the second,

cond, that is to say, for the intire refrigeration the relation given by the present experiments, being:: 35 + 47 + 56:31 + 43 + 50, or:: 138, 24, and :: 142: 125. By the preceding experiments, (Art. iv.) we shall have by adding the times, 280 to 249 for the still more precise relation of the entire refrigeration of iron and copper.

2. That the time of refrigeration of iron is to that of gold 451: 37 and to the point of temperature::

138:114.

3. That the time of refrigeration of iron fo as to be held in the hands, is to that of filver 45; 34,

and to the point of temperature : : 138:97.

4. That the time of the refugeration of iron, so as to be held in the hand is to that of tin::451:21 by the present experiments, and::24 11 by the preceding experiments, Art. V. Thus, by adding this time, we shall have, 691 to 32 for the still more precise relation of their refrigeration, and for the second, the relation given by the present experiments being::138:61, and by the preceding experiments (Art. v.)::136:73; we shall have, by adding these times, 274 to 134 for the still more precise relation of the entire refrigeration of iron and tin.

5. That the time of the first refrigeration of copper is to that of gold:: 40: 37, and:: 124: 114 for their entire refrigeration.

6. That the time of the first refrigeration of copper is to that of filver:: 401: 34, and :: 124: 97

for their entire refrigeration.

7. That the time of the first refrigeration of copper is to that of tin:: 401: 21 by the present experiments, and:: 431: 22 by the preceding ones (art. vi.) Thus we shall have by adding those times, 84 to 431 for the still more precise relation of their first refrigeration, and for the second the relation given by the present experiments being:: 124:61,

and :: 123: 71 by the preceding experiments (Art.vi) we shall have by adding these times, 247 to 123 for the ftill more precise relation of the entire refriges ration of copper and time

or 8. That the time of the first refrigeration of gold is to that of filver :: 37 : 34; and 1: 114: 97 for

their entire refrigerations on line coal to moissans

3 of That the time of the first refrigeration of gold is to that of the : 37 : 21, and : : 114 : 61, for their entire refrigeration.

20 10. That thertime of the first refrigeration of filver is to that of tin :: 34 : 21, and : : 97 : 61 for their

entire refrigeration; south some for the sale of the

to 22 for the fall

That the time of the relugication of iron,

at to be beid in the har IIX to that of tin : : ast : Having placed in the fame oven five bullets, their refrigeration was in the following proportions:

Gooled so as to be held in the	Gooled to the temperature:
-og hand half u ferends	the fecond, the relation g
Antimony in 6	In 2 2 2 2 25
Bilmuth in - 101 - 7	In + - 26
Lead in - comas 8	in o ota lo admit sa chio 27.
Zinc in 101	In 30
Emerald in 111	io well shirt in 38

Having repeated this experiment with a stronger degree of heat, and in which tin and bilmuth melted. the other bullets cooled in the following progression,

So as to be held in the hand	To the temperature.
and half a fecond.	sories and a 420 ca
Antimony in - 7.	In 28
Zinc in 14	In bus as as 44
Emerald in = 16	In In

That the name of .VIX

In the fame oven, and in the fame manner another bullet of Bismuth was placed, with fix other bullets which cooled in the following progression:

So as to be held in the hand.	To the temperature. Min.
Antimony in - 6	In: hele miles e 22
Bismuth in 6.	Impgersenentaliza
Lead in the transfer - 175	In to sale and Te. 28
Silver in 91	
Zinc in 101	
Gold in 111	
Emery 131	

XV.

Having repeated this experiment with the fame feven bullets, they cooled in the following order.

So as to be held in the band.	
Antimony in 61	In 23
Bismuth 71	In 31
Lead $7\frac{1}{2}$	In 29
Silver $11\frac{1}{2}$	In 32
Zinc 131	In 38
Gold 14	In 41
Emery 15.	In 44

All these experiments were made with care, and in the presence of two or three persons, who like me, judge by the touch, and holding the different bullets in their hands for half a second, therefore it may be concluded,

r. That the time of the first refrigeration of emery is to that of gold:: $28\frac{1}{2}$: 25, and:: 83:73 for their entire refrigeration.

Vol. V. L. 2. That

2. That the time of the first refrigeration of emery is to that of zinc:: 56: 48½, and:: 171: 144 for their entire refrigeration.

3. That the time of the first refrigeration of emery is to that of filver: : 28½: 21, and:: 83: 62

for their entire refrigeration.

4. That the time of the first refrigeration of emery is to that of lead:: 56: 32½, and:: 171: 123 for their entire refrigeration.

5. That the time of their first refrigeration of emery is to that of bismuth:: 40: 201, and:: 121:

80 for their entire refrigeration.

6. That the time of the first refrigeration of emery is to that of antimony:: $56:26\frac{1}{2}$, and :: to the temperature:: 171:99.

7. That the time of the first refrigeration of gold is to that of zinc:: 25:24, and:: 73:70 for their

entire refrigeration.

8. That the time of the first refrigeration of gold is to that of filver: :25:21, and::37:34 by the preceding experiments (Art. xi.) Thus we shall have by adding these times, 62 to 55 for the more precise relation of their first refrigeration, and for the second the relation given by the present experiments, being::73:62, and::114:97 by the preceding experiments (Art. ix.) we shall have, by adding these times, 187:159 for the more precise relation of their entire refrigeration.

9. That the time of the first refrigeration of gold is to that of lead :: 25:15, and :: 73:57 for their

entire refrigeration.

is to that of bismuth:: 25: 13½, and:: 73:56

for their entire refrigeration.

11. That the time of the first refrigeration of gold is to that of antimony: :25:12½, and::73:46 for their entire refrigeration.

12. That

12. That the time of the first refrigeration of zinc is to that of silver:: 24:21, and:: 70:62 for their entire refrigeration.

13. That the time of the first refrigeration of zinc is to that of lead:: 48: \(\frac{1}{2} \) 32\(\frac{1}{2}\), and:: 144:

123 for their entire refrigeration.

14. That the time of the first refrigeration of zinc is to that of bismuth: $34\frac{1}{2}$: $20\frac{1}{2}$, and: : 100: 80 for their entire refrigeration.

15. That the time of the first refrigeration of zinc is to that of antimony:: 48½; 26½, and to the

temperature : : 144 : 99.

16. That the time of the first refrigeration of silver is to that of bismuth: : 21:13½, and 62:56 for their entire refrigeration.

17. That the time of the first refrigeration of silver is to that of antimony:: 21: 12½, and:: 62:

46 for their entire refrigeration.

18. That the time of the first refrigeration of lead is to that of bismuth: 23: 20½, and 84: 80 for their entire refrigeration.

19. That the time of the first refrigeration of lead is to that of antimony:: 32½: 26½, and to the tem-

perature : : 123 : 99.

ding their times,

20. That the time of the first refrigeration of bifmuth is to that of antimony: : 201 : 191, and::

80: 71, for their entire refrigeration.

I must observe that in general in all these experiments, the sirst relations are much more just than the last, because it is difficult to judge of refrigeration to actual temperature, and that this temperature being variable, the result must also vary, whereas the point of the sirst refrigeration may be caught just enough by the sensation, which the heat of the bullet produces on the hand, where it can be held or touched for half a second together.

XVI.

As there were only two experiments for the comparison of gold with emery, zinc and lead, bismuth and antimony, that the bismuth being entirely melted, and the lead and antimony very much damaged: I made use of the bullets of bismuth, antimony and lead; and, I made a third experiment, by putting together these two bullets in the same wellheated oven; they cooled in the following order:

So as to be held in the hand half a second.	To the temperature.
Antimony in 7	In 27
Bismuth in Lead in 9	In 20 In 33
Zinc in 12	In 37
Gold in 13 Emery in 15½	In 42 In 48

From whence it may be concluded, 1. That the time of the first refrigeration of emery is to that of gold :: 44 : 38, and to the temperature :: 131 : THE LAST OF

2. That the time of the first refrigeration of emery is to that of zinc :: 151: 12; but the relation found by the preceding experiments (Art. xv.) being :: 56; 481, we shall have by adding these times, 711 to 601 for their first refrigeration; and for the fecond, the relation found by the preceding experiments (Att. xv.) as 171 to 144; thus, by adding these times, we shall have 239 to 181 for the still more precise relation of the entire refrigeration of emery and zinc.

2. That the time of the refrigeration of emery is to that of lead:: 15 ? : 9 by the relation found by the preceding experiments (Art. xv.) being:: 56: 321; thus we shall have, by adding these times,

713

71½ to 41½ for the more precise relation of their first refrigeration; and for the second, the relation given by the preceding experiment being::48:33, and by the preceding experiments (Art. xv.)::171:223, we shall have, by adding these times, 239 to 156 for the still more precise relation of the entire refri-

geration of emery and lead.

4. That the time of the refrigeration of emery is to that of bismuth: 15½:8, and by the preceding experiments (Art. xv.)::40:20½; thus we shall have, by adding these times, 55½ to 28½ for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiments being::48:29, and::121:80 by the preceding experiments (Art. xv.) we shall have, by adding these times, 169 to 109 for the still more precise relation of the emery and bismuth.

5. That the time of the first refrigeration of emery is to that of antimony: 15½:7; but the relation given by the preceding experiments (Art. xv.) being::56:26½, we shall have, by adding these times, 219 to 126 for the still more precise relation of the entire refrigeration of emery and antimony.

6. That the time of the first refrigeration of gold is to that of zinc:: 38:36, and::115:107 for

the entire refrigeration.

7. That the time of the first refrigeration of gold is to that of lead: 38:24, and to the temperature: 115:90.

8. That the time of the first refrigeration of gold to that of bismuth: : 38:21½, and to the tem-

. Frature : : 115 : 85.

7. That the time of the first refrigeration of gold is that of antimony:: $38:19\frac{7}{2}$, and to the tem-

perure :: 115:69.

is to at of lead: 12:9; but the relation found by the preding experiments (Art. xv.) being: 484

: 32½, we shall have, by adding these times, $60\frac{1}{2}$ to 41½ for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment:: 37: 33, and by the preceding experiments (Art. xv.):: 144: 133; we shall have, by adding these times, 181 to 156 for the still more precise relation of the entire refrigeration of zinc and lead.

11. That the time of the first refrigeration of zinc is to that of bismuth: 12:8 by the present experiment; but the relation found by the preceding experiment being (Art. xv.): 34½: 20½; by adding these times, we shall have 46½ to 28½ for the more precise relation of their refrigeration; and for the second, the relation given by the present experiment being: 37: 29, and by the preceding experiments (Art. xv.):: 100:80; we shall have, by adding these times, 137 to 109 for the still more precise relation of the entire refrigeration of zinc and bismuth.

12. That the time of the first refrigeration of zinc is to that of antimony: :12:7 by the present experiment; but as the relation found by the preceding experiments (Art. xv.) is ::48½:26½, we shall have, by adding these times, 60½ to 33½ for the still more precise relation of their first refrigeration; and for the second, the relation given by the present experiment being::37:27, and::144:99 by the preceding experiments (Art. xv.) we shall have, by adding these times, 181 to 126 for the more precise relation of the entire refrigeration of zinc and antimony.

13. That the time of the first refrigeration of ad is to that of bismuth: 9:8 by the present everiment, and: 23:20½ by the preceding one Art.

xv.) thus we shall have, by adding these imes, 32 to 28½ for the more precise relation of sir first refrigeration; and for the second, the relatin given

by the preceding experiments (Art. xv.) we shall have, by adding these times, 117 to 109 for the more precise relation of the entire refrigeration of lead and bismuth.

14. That the time of the first refrigeration of lead is to that of antimony :: 9:7 by the prefent experiment, and :: 321 : 261 by the proceding experiments (Art. v.): thus, we shall have, by adding these times 41 to 33 for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment :: 33: 27. and :: 123: 99 by the preceding experiment. (Art. xv.) we shall have, by adding these times 156 to 126 for the more precise relation of the entire re-

frigeration of lead and antimony.

15. That the time of the first refrigeration of bifmuth is to that of antimony :: 8:7 by the present experiment, and :: 2012: 19 by the preceding ones (Art. xv.) thus we shall have, by adding these times 281 to 26 for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment, being :: 29:27, and :: 80:71 by the preceding, we shall have, by adding these times 109 to 98 for the more precise relation of the entire refrigeration of bismuth and antimony.

XVII.

As there were but two experiments for the comparison of filver with emery, zinc, lead, bismuth, and antimony, I made a third, after the preceding methods; they cooled in the following order:

So as to be held in the hand half a second.				To the temperature.						
ilai)	4,000	,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,,	Min.	ar picula	*15H 40		Accept	Min.		
Antimony	in	000	6	In		no Alice		29		
Bifmuth in		1	7	In	-	40	21.0	31		
Lead in		•	81	In			dia To	34		
Silver in			III	In			0 5 70	36		
Zinc in	-		121	In	-	-	DIA.	36		
Emery in		-	151	In		-		47		
							F	rom		

From this experiment it must be concluded, and from those of articles xv. and xvi. 1st. That the time of the first refrigeration of emery is to that of zinc, by the present experiment, :: 15½: 12½, and :: 7½: 60½ by the preceding (Art. xvi.) thus, we shall have, by adding these times 83 to 73 for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment, being :: 47 to 39, and by the preceding ones, (Art. xvi.) :: 239: 181, we shall have, by adding these times 286 to 220 for the more precise relation of the entire refrigeration of emery and zinc.

2. That the time of the first refrigeration of emery is to that of filver:: 44:321, and:: 130:90

for the entire refrigeration.

3. That the time of the first refrigeration of emery is to that of lead:: 15½: 8½ by the present experiment, and:: 71½: 41½ by the preceding experiments (Art. xvi.) thus we shall have, by adding these times 87 to 49¾ for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment being:: 47: 34, and:: 239: 156 by the preceding experiments (Art. xvi.) we shall have, by adding these times 286 to 190 for the more precise relation of the

entire refrigeration of emery and lead.

4. That the time of the first refrigeration of emery is to that of the refrigeration of bismuth:: 15½: 7 by the present experiment, and:: 55½: 28½ by the preceding experiments (Art. xvi.) thus we shall have, by adding these times 71 to 35½ for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment being:: 47:31, and:: 169 to 109 by the preceding experiments; we shall have, by adding these times 216 to 140 for the more precise relation of the emery, and of bismuth.

5. That

5. That the time of the first refrigeration of the emery is to that of antimony :: 151:6 by the prefent experiment, and :: 711 : 331 by the preceding ones; thus, by adding these times, we shall have 87 to 391 for the more precise relation of their refrigeration; and for the second, the relation given by the present experiment, being:: 47:29, and by the preceding:: 219: 126, we shall have, by adding these times, 266 to 155, for the more precise relation of the entire refrigeration of emery and antimony.

6. That the time of the first refrigeration of zinc is to that of filver:: 3612: 321, and :: 109: 98 for

their entire refrigeration.

7. That the time of the first refrigeration of zine is to that of lead :: 121 : 84 by the present experiment, and :: $60\frac{1}{2}$: $4\frac{1}{2}$ by the preceding; we shall have, by adding these times, 73 to 433, for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment being:: 39: 33, and by the preceding :: 181: 156; we shall have, by adding these times, 220 to 189, for the more precise relation of the entire

refrigeration of zinc and lead.

e

at

8. That the time of the refrigeration of zinc is to that of the refrigeration of bismuth:: 121:7 by the present experiment, and 461: 281 by the preceding; therefore, we shall have, by adding these times, 59 to 351, for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment being: : 39 : 31, and :: 137: 109 by the preceding; we shall have, by adding these times, 176 to 140, for the more precise relation of the entire refrigeration of zinc and bifmuth.

9. That the time of the refrigeration of zinc is to that of antimony :: 121 : 6 by the present experiment, and :: 60½: 33½ by the preceding; thus VOL. V. we

we shall have, by adding these times, 73 to 39½, for the more precise relation of their first refrigeration; and for the second, the relation found by the present experiment being:: 39:29, and :: 181:126 by the preceding experiments; we shall have, by adding these times, 220 to 155, for the more precise relation of the entire refrigeration of zinc and antimony.

10. That the time of the refrigeration of filver is to that of lead: :32½: 23¼, and ::98:90 for their

entire refrigeration.

ver is to that of bifmuth:: $32\frac{1}{2}$: $20\frac{1}{2}$, and:: 98: 87 for their entire refrigeration.

12. That the time of the refrigeration of filver is to that of antimony:: 32½: 18½, and:: 98:75

for their entire refrigeration.

13. That the time of the refrigeration of lead is to that of bismuth:: $8\frac{1}{4}$: 7 by the present experiment, and:: $32:28\frac{1}{2}$ by the preceding; we shall have, by adding these times, $40\frac{1}{4}$ to $35\frac{1}{2}$, for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment being:: 34:31, and::117:109 by the preceding; we shall have, by adding these times, 141 to 140, for the more precise relation of the entire refrigeration of lead and bismuth.

14. That the time of the refrigeration of lead is to that of antimony:: $8\frac{1}{4}$: 6 by the prefent experiment, and by the preceding:: $41\frac{1}{2}$: $33\frac{1}{2}$; thus we shall have, by adding these times, $49\frac{3}{4}$ to $39\frac{1}{2}$, for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiment being:: $34 \cdot 29$, and:: 156: 126 by the preceding; we shall have, by adding these times, 190 to 155, for the more precise relation of the entire

refrigeration of lead and antimony.

15. That

is to that of antimony::7:6 by the present experiment, and::28½:26 by the preceding; thus we shall have, by adding these times; 35½:32, for the more precise relation of their first retrigeration; and for the second, the relation given by the present experiment being::31:29, and::109:98 by the preceding; we shall have, by adding these times, 140 to 127, for the more precise relation of the entire refrigeration of bismuth and antimony.

XVIII.

There was put in the same oven a bullet of glass, another of tin, one of copper, and one of iron, and they cooled in the following order:

Cooled fo as	tol	e h	eld i	in the	Cooled to the temperature						
hand for	baij	a,	econ	Min.	148		-		*	ins	Mint
Tin in Glass in		-	-	81	In				- 1	ija	27
Copper in	-			14	In			-			42
Iron in			-	16	In	-		-			.50

a now to compagnion XIX. san to other add

The fame experiment being repeated, cooled in the following order.

Cooled so as to be held in the hand for half a second.	Cooled to thetemperature.
Min.	.nild prelebters personent abe
Tin in $7\frac{1}{2}$	
Glass in - 8	
Copper in 12	In - 36
Iron in 15	In 47

ban XX. rds rangers to Jeat at

By a third experiment, the bullet being kept hot

a longer time, but in a flower heat, cooled in the following order:

So as to be beld in the hand	To the temperature.
half a second. firm a	more precise relation of
The important year Min.	for Mbe recond, the relati
Tin in 81 I	n goise asmi??
Glass in 9 In Copper in 15 I	10. 10. 17. 17. 17. 17. 17. 17. 17. 17. 17. 17
Iron in - , - 17 I	n 7 7 7 7 7 43

XXI.

By a fourth experiment, the same bullets heated by a fierce fire, cooled in the following order:

Cooled so as to be held in the hand half a second.	Cooled to the temperature.
Min.	Min.
Tin in $-$ - $8\frac{1}{2}$	In 25
Glass in 9	In 25
Copper in $-11\frac{1}{2}$	In 35
Iron in 14	In 43

From these repeated experiments there results, 1. That the time of the first refrigeration of iron is to that of copper:: 62:52½ by the present experiment, and::99:85½ by the preceding (Art. xi.) thus we shall have, by adding these times, 161 to 178, for the more precise relation of their first resrigeration; and for the second, the relation given by the present experiment being::186:156, and by the preceding::280:249; we shall have, by adding these times, 466 to 405, for the more precise relation of the entire refrigeration of iron and copper.

2. That the time of the refrigeration of iron is to that of copper:: 62:34½, and::186:97 for

their entire refrigeration.

3. That the time of the refrigeration of iron is to that of tin::62:32½ by the prefent experiments, and::69½:32 by the preceding; thus we shall have, by adding these times, 131½ to 64½, for the more precise relation of their first refrigeration; and for the second, the relation given by the present experiments being::186:92, and::274:134 by the preceding; we shall have, by adding these times, 460 to 226, for the more precise relation of the entire refrigeration of iron and tin.

4. That the time of the refrigeration of copper is to that of glass: $51\frac{1}{2}:34\frac{1}{2}$, and :: 157: 97 for

their entire refrigeration.

5. That the time of the refrigeration of copper is to that of tin:: $52\frac{1}{2}:32\frac{1}{2}$ by the present experiments, and:: $84:43\frac{1}{2}$ by the preceding; thus we shall have, by adding these times, $136\frac{1}{2}$ to 76 for the precise relation of their first refrigeration; and for the second, the relation given by the present experiment being:: 157:92, and by the preceding:: 247:132; we shall have, by adding these times, $136\frac{1}{2}$ to 76, for the precise relation of their first refrigeration; and for the second, the relation given by the present experiment being:: 157:92, and by the preceding:: 247:132; we shall have, by adding these times 304 to 224 for the more precise relation of the entire refrigeration of copper and tin.

6. That the time of the refrigeration of glass is to that of tin:: 24½: 32½, and :: 97: 92 for their

entire refrigeration.

1. That the time of the HXX

Bullets of gold, glass, porcelain, gypsum, and gres, were heated together, and cooled in the following order:

Cooled so as to be held in the Cooled to actual temperature. hand for half a second.

A 10 00 10 11			Min.	13/65-30	STREET ON A LEE	Min.
Gypsum in -	-	50	8	In	: สาดเลียวายอ	- 44
Porcelain in	-	-	8 8 <u>1</u>	In	ורכינורופינו	- 25
						Class

Glass in Gress in Gold	In the strip of the 26
Gres in Square 10	In leg : so : nin to BE:
Gold in w and ; gnid 41	10 di vo se : 100 : 45

have, by adding these ti

The same experiment repeated on the same bullets, cooled in the following order:

Cooled fo as to be held in the hand half a second.	Cooled to the temperature.
Gypfum in 2 doi 10 4 Porcelain in 7	That the time of the
Glass in Gress in Gre	In other jenigeration of the
Gold in toolore 2dt 131	this of the safet mi

thell have, by adding . VKK mes, 136 to 76 for

The same experiment repeated, the bullet cooled in the following order:

Godled so as to be beld in the	Cooled to the temperature.
hand half a second.	Com the cast for the fecon
Gypfum in - 21	dinomison harmined 12
Porcelain in 1 51	In the state of the
Glass in ser- stor- of 81	
Gres in 84	Ingil
Gold in la - non- no	In 10 - 32

From these three experiments there results, 1. That the time of the refrigeration of gold is to that of gres :: 38: 28, and :: 118: 90 for their entire refrigeration.

2. That the time of the refrigeration of gold to that of glass is :: 38: 27, and :: 118: 70 for their

entire refrigeration.

cal store of their

3. That the time of the refrigeration of gold is to that of porcelain :: 38:21, and :: 118:66 for their entire refrigeration,

4. That the time of the refrigeration of gold is to that of gypfum: 38:12½, and ::118:39 for their entire refrigeration.

5. That the time of the refrigeration of gres is to that of glass: 28½: 27, and: :90: 70 for their

entire refrigeration.

6. That the time of the refrigeration of gres is to that of porcelain: 28½; 21, and :: 90: 66 for their entire refrigeration.

7. That the time of the refrigeration of gres is to that of gypfum: 28½: 12½, and: 90: 39 for their

entire refrigeration. To nothing a letterized

8. That the time of the refrigeration of glass is to that of porcelain: 27:21, and::70:66 for their entire refrigeration.

9. That the time of the refrigeration of glass is to that of gypsum: 27:121, and: 70:39 for their

entire refrigeration.

10. That the time of the refrigeration of porcelain is to that of gypsum: :21:12½, and::66:39 for their entire refrigeration.

XXV.

Bullets of filver, common marble, hard stone, white marble, and fost calcareous stone of Amienes, near Dijon, were heated like the former.

Cooled fo as to be held in the band half a second.	C	oolea	l to	tel	mper	atu	re.
Min.						To the	Min.
Soft calcareous stone in 8	In		-49	1		-	25
Hard stone in - 10	In					-	34
Common marble in 11		1	1-		-	-	35
White marble in - 12	In	-		-	1	-	36
Silver in 1312	In				4	-	40

-d 32 d

Poblicy Leaves . . . record to lear-mark &

XXVI.

The same experiment repeated, cooled as follows:

Cooled so as to be held in the	Cooled to the temperature.
hand half a second.	es - 12s s vieto to tent
Min.	Min.
Soft calcareous stone in 9	In - 101 - 27
Hard ditto in	In 10-20013-01 30-1 -37
Common marble in 13	In 3 - 104 3 - 9 - 40
White marble in - 14	In
Silver in - 1-11-116	In - 43

As the continual repetition of these experiments are very long and no doubt tirefome to the reader, we shall here give the general table of these relations all compared to 10000, so that the differences may be perceived at one view.

BL

Of the Relations of different Mineral Substances.

IRON.

							Entire
	1			Firft	Refriger	atio	n. Refrigeration.
Emery					10000	te	9117-9020.
Copper					10000	to	8512-8702.
Gold .	. n.				10000	to	8160-8148.
Zinc .	16.01		•	3 3	10000	to	7654-6020.
Silver .	500			Dil	10000	to	7619-7423
Marble w	white				10000	to	6774-6704.
Marble c	omm	on	1.				6636-6746.
Stone cal	careo	us	h	ard	10000	to	6617-6274.
Gres					10000	to	5796-6926.
Glass .							5576-5805.
Lead .							5143-6482.
Tin .				1150			4898-4921.
Stone cal	care	ous	fo	ft			4194-4659.
Clay .							4198-4490.
Bifmuth	-			10			3580-4081.
Chalk .							3086-3878.
Gum .							2325-2817.
Wood .							1890-1594
Pumice	ftone						1627-1268.
			1				E M E-

Iron and

1 0.15.	E	M	E	R	Y	· Sec
and subjector and	ipera i	ar arb	n.H.	1	Be Keley	FirR
- Collemagne	021000	214	4 .	E	ntire retrig	to 8519—8148.
112	Copp	er				
	Gold					to 8513—8560.
1207-1289	Zinc	15 ·			10000	to 8390-7692.
Enlery and	Silver	55 X	1:01:	Heb!	10000	to 7778—7895.
VICES TAFRE	Stone	calca	tenne	har	1 10000	to 7304—6963.
\$658	Gres	Carca	ircous	ilai.	10000	to 6552-6517.
5325-526.30	Glass	tot		3 1	10000	to 5862—5506.
r	Lead	5:	11. 1			to 5718-6643.
Emery and	Zinc	100	130 10	-	10000	to 5658—6000.
		10.	1 1	-		
the second	Clay			: :		to 5185—5185.
	The Additional Control of the Contro					to 4949—6060.
	Antin	lony	63.03	I		to 4540—5827.
	Oker					to 4259—3827.
2	Chalk					to 3684—4105.
1007	Gypfi					to 2368-2947.
Sess-Sess.	Wood			:	. 10000	to 1552—3146.
- 1117 - 5510 - 8683	C	0	P .	P	E R.	o to 9136—9194.
P DADA	Zinc					o to 8571—9250.
0129-1556	Silver	Yu E	1 10		. 1000	o to 8395—7823.
01/01/01			mmo	1	. 10000	to 7639—8019.
	Gres	1				o to 7333—8160.
1000-000	Glafs	002			1000	to 6667—6567.
6000000	Lead	100	100			
Copper and	Tin	395			1000	to 6179—7367. to 5746—6916.
48.84		cale	areous	toni	der 1000	o to 5168—5633.
(1000) ONL)	Clay	004		, cem		o to 5652-6365.
	Bifm	ath.			1000	o to 5680—5959.
	Anti				1000	o to 5130—5808.
	Oker			e i	1000	o to 5003-4697.
200	Chal		ALC: IL		1000	o to 4068—4368.
	Cuar			N		0 10 4000-4300.
ALCOHOLD STATE		G	0	L	D.	
100 - 100 m	Zind			3701	: 1000	o to 2474-9304.
Gold and	Silve		band.	SHOW	. 1000	o to 8936—8686.
Sold alle	Mart		hite			o to 8101—7863.
TENET-OF			mmor	1		o to 7342—7435
binf			çareou		rd 1000	o to 7383-7516.
Vol. V.				N		Gres

.7.	MIT E M E R Y
A PART OF THE PART	Entire refrigeration. refrigeration.
Leo responsibility	
gire-bres	Glafs 10000 to 7103-5232.
100000000000000000000000000000000000000	Lead 10000 to 6526-7500.
\$200 ye.c.	Tin 10000 to 6324-6051.
111	Stone calcareous foft 10000 to 6087-5811.
1364133531	Clay 10000 to 5814-5077.
Gold and .	Bismuth 10000 to 5658-7043.
1000	Porcelain 10000 to 5526-5593.
30.3-4003	Antimony 10000 to 5395-6348.
	Oker 10000 to 5349—446z.
	Chalk 10000 to 4571—4452.
	Gypsim 10000 to 2989—3293-
. Charmentall	(C) primir 10000 to 2909—3293.
THE RESIDENCE AND	Z I N C.
and the second	
1603-600	/Silver 10000 to 8904-8990.
100-400	10015
	Marble white 10000 to 8305-8424.
	7194
	Gres 10000 to 6242-7333.
	5838
TO SERVICE SER	Lead 10000 to 6051-7947.
Service Service	Tin : 10000 to 6777—6240.
	Tin : 10000 to 6777—6240.
	Stone calcareous fost 10000 to 5536-7719.
Zinc and .	4425
	Clay 10000 to 5484-7458.
A STATE OF THE STATE OF THE	4573
	Bismuth 10000 to 5343-7547.
	4232
	Antimony , . 10000 to 5246-6608.
	Chalk 10000 to 3729—5862.
and the later of	2618
	Gypfum 10000 to 3409-4261.
	1298.
A STATE OF THE PARTY OF THE PAR	
	SILVER.
	Marble white 10000 to 8681-9200.
	Marble common 10000 to 7012-0040.
Silver and .	Stone calcareous hard 10000 to 7436-8580.
	Gres 10000 to 7361-7767.
	Glass 10000 to 7230-7212.
	Lead

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Benf Reference	Entire Refrigeration, Refrigeration.
· to adde-safeog. V	Entire Kerrigeration, Kerrigeration.
	read 10000 to 7154—9104.
A STATE OF THE RESIDENCE OF THE PARTY OF THE	rin : 10000 to 6176—6289.
	Stone calcareous foft 10000 to 6178—6289.
Library - Sounds	Clay 10000 to 6034—6710.
	Bismuth 10000 to 6308—8877.
Silver and . [1	Porcelain 10000 to 5556-5242.
Silver and .	Antimony 10000 to 5692-7653.
	Oker 10000 to 5000—5658.
A	Chalk 10000 to 4310—5000.
And the second s	Gypsum 10000 to 2879—3366.
	Wood 10000 to 2353—1864.
A Marie Mari	Pumice-stone 10000 to 2059-1525.
11.60a18-3028-1	Tunnee-none 10000 to 2039—1323.
N	ARBLE WHITE.
Cd - Cd	Marble common . 10000 to 8992-9405.
	Stone hard 10000 to 8594—9130.
	Gres 10000 to 8286-8990.
	Lead 10000 to 76045555,
Beliefier	Tin 10000 to 71436792.
Marble	Stone calcareous foft 10000 to 67927218.
white and	Clay 10000 to 64006286.
	Antimony 10000 to 6286-6792,
110000000000000000000000000000000000000	Oker 10000 to 5400-5571.
20 10 10 10 10 10 10 10 10 10 10 10 10 10	Gypsum 10000 to 4920—5116.
-5010 S010	
.110000110	Wood 10000 to 2200—2857.
·M	ARBLE COMMON.
	(Stone hard 10000 to 9483-9644.
	Gres 10000 to 8767—9273.
1. 人民國知論學學	Lead 10000 to 7671—8590.
THE TOTAL THE SERVICE	Tin , 10000 to 7424—6666.
Marble	Stone fost 10000 to 7327—7959.
common and	
. 1012-111 8	Antimony 10000 to 62798333.
istor bond	Oker 10000 to 61366393.
11100-1102	Chalk 10000 to 55816333.
Translation .	Wood 10000 to 25003279.
ST	ONE CALCAREOUS HARD.
	Gres 10000 to 92689355.
Stope hard &	Glass 10000 to 87108352.
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	of opposit

A 17 11		. 1	Entir	e Refriger	ratio	n. Refrigeration.
. Mottney Million.	Tin .			10000	to	8095 7931.
4870-5510	Stone foft			10000	to	80008095.
Stone hard &	Clay			10000	to	61906897.
	Oker)1 8510	07207	10000	to	47625517.
-0170-0199	Wood		•	10000	to	21954516.

GRES

	Glass .					10000	to	9324-7939.	
*DODE-1-101-5#	Lead .		•		•	10000	to	8561-8950.	
·0018013	Tin .					10000	to	7667-7633.	
MARKET CERT	Stone foft				•	10000	to	7647-7193.	
Gres and . (Porcelain				• **	10000	tó	7364-7059.	
	Antimeny					10000	to	73336170.	
	Gypfum	445				10000	to	45685000,	
Bygz Ojor.	Wood .		. 00	icu				23684828,	

COPPS—COLD IN CG L A S S.

71436702	/Lead .		*			10000	to	93188548,
8107	Tin .	13		RES	TLAS	10000	to	91078679.
\$4000apd	Clay .			:		10000	to	79387643.
Glass and . <	Porcelain					10000	to	76928863.
ains and 19	Oker .					10000	to	6289 6500.
4920-5116.	Chalk	:	:			10000	to	61046195.
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	Wood				*	10000	to	26475514
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I STONE CAMAINGUS HIRED.

. safet bird	Clay			10000	to	88239524.
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	State March Land	W. Car	C. D.	ion. Refrige	
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Avious spring	SOker Chalk	500000		5882	
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GYPSUM.

Gypsum and \{\begin{array}{llll} \text{Wood} & \text{. } & \text{I0000 at 8000---5260.} \\ \text{Pumice-stone} & \text{. } & \text{I0000 to 7000---4500.} \end{array}

WOOD.

Pomice-stone : 10000 to 8750---8182.

However great affiduity I have used in my experiments, whatever care I had taken to render the relations more exact, I own that there are still some imperfections in this Table which contains the whole; but these defects are trivial, and not much influence the general refults: for example, we shall easily perceive, that the relation of zinc to lead being 10000 to 6051, that of zinc to tin should be less than 6000; whereas it is found 6777 in the Table. It is the fame with respect to filver to bismuth, which ought to be less than 6308, and still more with regard of lead to clay, which ought to be more than 8000, but in the Table only 7878; but this proceeds from the leaden and bifmuth bullets being not always the fame; they melted as well as those of tin and antimony, which could not fail of producing variations, the greatest of which are the three which I have just remarked. It was not possible for me to do better: the different bullets of lead, tin, bismuth, and antimony, which I fuccessively made use of, were made, in fact, in the fame manner, but the matter of each might be fomewhat different, according to the' quantity of the alloy in the lead and tin; for I had only pure tin for the two first bullets: besides, there remains very often a fmall cavity in the melted bullet, and these little causes are sufficient to produce the little differences which may be remarked in the Table.

On the whole, to draw from these numerous experiments all the profit that can be expected, the matters matters which compose the object must be divided into four classes, or different genus's.

rals. 2. Semi metals and metallic minerals. 3. Vitreous and vitrescible substances. 4. Calcareous and calcinable substances. To compare afterwards the matters of each genus between themselves, to discover the cause or the causes of the order which follows the progress of heat in each, and, at last, to compare the genus's between each other, in order to deduce some general results.

T.

The order of the fix metals, according to their denfity, is tin, iron, copper, filver, lead, and gold; whereas the order in which these metals receive and lose their heat is tin, lead, filver, gold, copper, iron, among which there is only tin which retains its place.

The progress and duration of heat in metals does not then follow the order of their density, if it was not for tin, which, being the least dense of all, is, at the same time, that which soonest loses its heat: but the order of the eight other metals demonstrates that it is in relation of their sufficient to melt than gold, gold more than silver, silver more than lead, and lead more than tin; we must, therefore, conclude, that it is only a chance if the density and sufficient to the last rank.

Nevertheless, it would be advancing too much to pretend that we must attribute all to suffibility, and nothing to density. Nature never deprives herself of one of her properties in favour of another in an absolute manner; that is to say, in a mode that the first had not any influence on the second. Thus, density may be of some weight in the progress of heat: but, at least, we cannot pronounce affirmatively.

tively, that it is of very little in the fix metals;

whereas fufibility does almost all:

This first truth was neither known to the chemists nor physicians; they did not even imagine that gold, which is more than twice as dense as iron, nevertheless, loses its heat near a third sooner. It is the same with lead, silver, and copper, which are all denser than iron, and which, like gold; heat and cool more readily; for, although the refrigeration in my second Memoir is only questioned, the experiments of the preceding Memoir demonstrate, that it is not to be doubted that there is an ingress of heat into bodies as well as its egress, and that those which receive it the quickest, are, at the same

time, those that lose it the foonest.

If we reflect on the real principles of denfity, and the cause of fusibility; we shall perceive, that denfity depends absolutely on the quantity of matter which Nature places in a given space; that the more the can make it enter therein, the more density there will be; and that gold, in this respect, is of all the fubflances, that which contains the most matter relatively to its volume. It is for this reason that it has been hitherto thought, that more time is required to heat or cool gold than other metals: it is; in fact, natural enough to think, that containing double or treble matter under the same volume; double or treble time would be required to penetrate it with heat; and this would be true, if in every fubstance the constituent parts were of the same figure, and in confequence of all, ranged the fame: But in some, as in the most dense, the molecules of matter are, probably, of a figure regular enough not to leave very void places between them: in others which are not so dense, and their figures more irregular, leave more numerous and large vacuities; and in the lightest, the molecules being few; and, probably of a very irregular figure, a thousand and

a thousand times more void is found than plenitude; for it may be demonstrated by other experiments, that the volume of even the densest substance contains still much more void space than full matter.

Now, the principal cause of fusibility is the facility the particles of heat find in separating these molecules of full matter from each other; let the fum of the vacuities be greater or lefs, which causes density or lightness, it is indifferent to the separation of the molecules which constitute the plenitude; and the greater or less fusibility depends entirely on the power of coherence which retains these massive parts united, and oppose themselves more or less to their feparation. The dilatation of the total volume is the first degree of the action of heat; and in different metals it is made in the same order as the fusion of the mass, which is performed by a greater degree of heat or fire. Tin, which melts the readiest of all, is also that which dilates the quickest; and iron, which is the most difficult to melt, is likewise that whose dilatation is the flowest.

After these general notions which appear clear, precise, and founded on experiments which nothing can contradict, it might be imagined, that ductility follows the order of fufibility, because the greater or less ductility seems to depend on the greater or less adhesion of the parts in each metal; nevertheless, this order of ductility in metals, feems to have as much connection with the order of density, as that of their fufibility: I should readily say, that it is in a ratio composed of the two others, but that it is only by estimation and a presumption which is, perhaps, not founded; for it is not fo easy justly to determine the different degrees of fusibility, as those of denfity; and as ductility participates of both, and varies according to circumstances, we have not as yet acquired the necessary knowledge to pronounce affirmatively on this subject, which is of sufficient VOL. V.

importance to merit particular researches. The same metal treated cold or hot, gives quite different refults. Malleability is the first mark of ductility; but it, nevertheless, gives us only one notion, imper-fect enough, of the point to which ductility may extend: nor can fimple lead, the most malleable metal be drawn into fuch fine threads as gold, or even as iron, which is the least malleable of all. Befides, we must affist the ductility of metals with the addition of fire, without which they become brittle: even iron, although the most robust of all, is brittle like the rest. Thus, the ductility of one metal, and the extent of continuity which may support it, depends not only on its denfity and its fufibility, but also of the manner in which it is heated, of the flower or quicker percustion, and of the addition of heat or fire which is properly given to it.

II.

Now, if we compare the fubstances which we term semi metals and metallic minerals, which want ductility, we shall perceive, that the order of their denfity is emery, zinc, antimony, and bismuth; and that in which they receive and lose heat, is antimony, bismuth, zinc, emery, and which does not in any measure follow the order of their density, but rather that of their fufibility. The emery, which is a feruginous mineral, although once denfer than bifmuth, retains heat longer. Zinc, which is lighter than antimony and bismuth, retains also heat a much longer time. Antimony and bismuth, receive it and keep it nearly alike. There is, therefore, semi metals, and metallic minerals like metals: the relation in which they receive and lose heat, is nearly the same as that of their fusibility, and partakes very little or not at all of their denfity.

But by joining the fix metals, and the four femi metals, or metallic minerals, which I have tried, we shall find the order of the densities of these ten mineral substances to be emery, zinc, antimony, iron, copper, bismuth, filver, lead, and gold.

And that the order in which these substances heat and cool, is antimony, bismuth, tin, lead, filver, zinc, gold, copper, emery, and iron, in which there are two things that do not appear to agree

well with the order of fufibility.

First, Antimony, which should heat and cool flower than lead, fince we have feen, by the experiments of Newton quoted before, that antimony requires ten degrees of the same heat to fuse it, as lead which requires only eight; whereas, by my experiments, antimony is found to heat and cool quicker than lead. But we shall observe, that Newton made use of the regulus of antimony, and that I have employed only melted antimony in my experiments. Now, the regulus of antimony, or native antimony, is much more difficult to fuse, than antimony which has already undergone a first fusion; thus, that does not make an exception to the rule. On the whole, I do not know what relation native antimony, or regulus of antimony, may have with the other matters I have heated and cooled; but I prefume, after the experiments of Newton, that it heats and cools flower than lead.

Secondly, it is pretended, that zinc fuses more easily than silver; consequently, it should be found before silver in the order indicated by my experiments, if this order was in all cases relative to that of suspending and I own, that this semi metal seems, at the sirst glance, to make an exception to this law which all the rest follows: but it must be observed, that the difference given by my experiments between zinc and silver is very trisling. Secondly, That the small globe of silver which I made use of was of the purest silver, without the least mixture of copper. Thirdly, Although the little globe of zinc was given

me by one of our able chemists (Mr. Rouelle, professor of chemistry in the school of the king's garden), it perhaps was not absolutely pure zinc, without any mixture of copper, or of some other still less suffible matter. As this supposition remained to me after all my experiments, I returned the globe of zinc to Mr. Rouelle, requesting him to assure himself whether it did not contain iron, or copper, or some other matter which might oppose its suffibility. The trials having been made, Mr. Rouelle found a pretty considerable quantity of this iron, or saffron of steel, in this zinc.

I have, therefore, had the satisfaction to see that not only my supposition was well founded, but also that my experiments have been made with sufficient precision to evince a mixture. Thus, zinc also exactly sollows the order of suspility, like the other metals and semi metals in the progress of heat, and does not make any exception to the rule. It cannot, therefore, in general be said, that the progress of heat in metals, semi metals, and metallic minerals, is in the same ratio, or, at least, in a very near ratio

to that of their fufibility.

III.

The vitrescible, and vitreous matter which I tried, being ranged according to their density are, Pumice-stone, rock chrystal, and gres: for I must observe, that, although chrystal is not set down in the table of the weight of each matter but for 6 drams 22 grains, it must be supposed 1 dram heavier, because it was insensibly too small; and it is for this reason, that I have excluded it from the general table of relations; nevertheless, the general result agrees with the rest, so that I can present it. Here follows then the order in which these different substances are cooled.

Pumice-

Pumice-stone, oker, porcelain, clay, glass, chrystal, and gres, which, as is seen, is that of their density; for the oker is not here sound before the porcelain, because that being a sufible matter, it diminished by the friction it underwent in the experiments, and besides, its density differs so little from porcelain,

that it may be looked upon as equal.

Thus the law of the progress of heat in vitrescible and vitreous matters is relative to the order of their density, and has but little or no relation with their fusibility, by the reason required to sufe all these substances, which is an almost equal degree; and that the particular degrees of their different suffibility are so near each other, that a compound order of distinct terms cannot be made: thus, their almost equal suffibility, making only one term, which is the extreme of this order of suffibility, we must not be associated that the progress of heat here follows the order of density, and that these different substances, which are all equally difficult to sufe, heat and cool more slowly, and quicker in proportion to the matter which they contain.

It may be objected, that glass fuses more easily than clay, porcelain, oker, and pumice-stone, which, nevertheless, heats and cools in less time than glass; but the objection will fall, when we shall reslect, that to fule glass it is requisite to have a very fierce fire, the degree of which is fo remote from the degrees of heat which glass receives in our experiments on refrigeration, that it cannot have any influence on these. Besides, by powdering clay, porcelain, and pumice-stone, and giving them there analogous fusers, as we give to fand to convert it into glass, it is more than probable that we should fuse all the matters in the same degree of fire, and that, consequently, we must look upon it as equal or almost equal with their refistance to fusion; and, it is for this reason, that the law of the progress of heat in these matters matters is found proportionable to the order of their denfity.

IV.

Calcareous matters, ranged according to the order of their denfity are, chalk, foft stone, hard stone, marble common, and marble white, which is, as is feen, the same as that of their density. The fusibility does not enter therein as any thing, because it immediately requires a very great degree of fire to calcine them; and that, although the calcination divides the parts, we must look upon the effect only as a first degree of fusion, and not as a complete fusion, The whole power of the best burning mirrors is scarcely sufficient to perform it: I have found and reduced into a kind of glass some of these calcareous matters; and I am convinced that these matters may, like all the reft, be reduced ulteriorly into glass, without employing thereto any fusing matter, and only by the force of a fire quite superior to that of our furnaces; consequently, the common term of their fufibility is still more remote, and more extreme than that of vitreous matters, and it is for this reason that they also follow more exactly the order of denfity in the progress of heat.

White gyplum, improperly called alabaster, is a matter which calcines like all other plaisters, by a more moderate heat than that which is necessary for the calcination of matters calcareous; does it not follow the order of density in the progress of heat which it receives, or which it loses? for, although much more dense than the chalk, and a little denser than the white calcareous stone, it heats and cools, nevertheless, much more readily than the one or the other of those matters. This demonstrates to us, that the more or less easy calcination and susion produces the same effects relatively to the progress of heat. Gypsous matters do not require so much fire to calcine as calcareous matters, and it is for this

reason

reason that, although denser, they heat and cool

quicker.

Thus it may be affured in general, that the progress of heat in all mineral substances is always nearly in a ratio of greater or less facility to calcine, or melt that when their calcination, or their suspense equally difficult, and that they require a degree of extreme heat, then the progress of extreme heat, is made according to the order of their density.

On the whole, I have deposited in the Royal Cabinet, the globes of gold, silver, and of all other inetallic and mineral substances which have served for the preceding experiments, in order to render them more authentic, by inclining more to verify them who doubt of the truth of their results, and of the general consequence which I have deduced.

MEMOIR THE THIRD.

Observations on the Nature of PLATINA.

WE have just observed, that of all the mineral substances I have subjected to trial, those are not the most dense to which the longest time is required to receive and lose heat. Iron and emery, which are the most dissicult matters to suse, are, at the same time, those which heat and cool the slowest. There is in Nature only platina which is less accessible to heat, and which retains it longer than iron. This mineral, which has been publicly mentioned but a short time, appears to be more difficult to suse; the sire of the best surnaces is not sierce enough to produce that effect, nor even to agglutinate the simall grains, which are all angular, hard, and similar in form to the thick scale of iron, but of a yellowish

lowish colour; and although we can fuse them without any addition, and reduce them into a mass by a mirror, platina feems to require more heat than the ore and scales of iron which we easily fuse in our forge furnaces. In other respects, the density of platina being much greater than that of iron, the two quantities of denfity and non-fufibility unite here to render this matter least accessible of all to the progress of heat. I presume, therefore, that platina would be at the head of my Table if I had put it to the experiment; but I was not able to procure a globe of it of an inch diameter, it being only found in grains; and that which is in the mass is not pure, because other matters have been mixed with it in order to fuse it, which alters its nature. The count of Billarderie d'Angivilliers, one of my friends, and a man of great judgment, who did me the favour to attend often my experiments, led me to examine this rare metallic substance, not yet sufficiently known. Chemists, who have employed their time on platina, have looked upon it as a new, perfect, proper, and particular metal, different from all the rest: they have afferted, that its specifical weight was nearly equal to that of gold; that, nevertheless, this metal differed in other respects essentially from gold, having neither ductility nor fufibility. I own that I am of a quite contrary opinion: a matter which has neither ductility nor fufibility, must not rank in the number of metals, whose effential and common properties are to be ductile and fufible. Platina, after the examination I gave it, did not appear to be a new metal different from every other, but an alloy of iron and gold formed by Nature, in which the quantity of gold predominated over the iron; and here follow the facts on which I founded this opinion.

Of 8 ounces 35 grains of platina, furnished me by M. de Angivilliers, and which I presented to a

frong

ftrong loadstone, remained only i ounce, I dram, grains, all the rest was taken away by the loadstone; so that there remained but 2 drams, which was reduced into powder which stuck to paper, and blackened them, as I shall presently observe. This makes, therefore, nearly fix fevenths of the whole which was attracted by the loadstone, which is so considerable a quantity, that it is impossible to suppose that iron is not contained in the intimate substance of platina, but that it is even there There is more; for if. I in a very great quantity. had not been weary of these experiments which remained several days, I should have still attracted a great part of the remainder of the 8 ounces by my loadstone; for the loadstone still attracted some grains one by one, and sometimes two. There is, therefore, much iron in platina, and it is not fimply mixed therewith as a foreign matter, but intimately united and making part of its substance; or, if this is denied, it must be supposed, that there exists a second matter in Nature which is attractable by the loadstone, like iron: but this supposition will be overthrown by other circumstances I shall relate.

All the platina I have had an opportunity to examine, has appeared to be mixed with two different matters, the one black, and very attractable by the loadstone; the other in larger grains, of a pale yellow, and much less magnetic than the first. Between these two matters, which are the two extremes of this kind of mixture, is found all the intermediate links, whether with respect to magnetifm, colour, or fize of the grains. The most magnetic, which are, at the same time, the blackest and smallest, reduce easily into powder by a very flight friction, and leaves on white paper the fame mark as lead? Seven leaves of paper which were fuccessively made nse of to expose the platina to the action of the loadstone, were blackened over the whole extent occu-VOL. V. pied lowish colour; and although we can fuse them without any addition, and reduce them into a mass by a mirror, platina feems to require more heat than the ore and scales of iron which we easily fuse in our forge furnaces. In other respects, the density of platina being much greater than that of iron, the two quantities of denfity and non-fufibility unite here to render this matter least accessible of all to the progress of heat. I presume, therefore, that platina would be at the head of my Table if I had put it to the experiment; but I was not able to procure a globe of it of an inch diameter, it being only found in grains; and that which is in the mass is not pure, because other matters have been mixed with it in order to fuse it, which alters its nature. The count of Billarderie d'Angivilliers, one of my friends, and a man of great judgment, who did me the favour to attend often my experiments, led me to examine this rare metallic substance, not yet sufficiently Chemists, who have employed their time known. on platina, have looked upon it as a new, perfect, proper, and particular metal, different from all the rest: they have afferted, that its specifical weight was nearly equal to that of gold; that, nevertheless, this metal differed in other respects effentially from gold, having neither ductility nor fulibility. I own that I am of a quite contrary opinion: a matter which has neither ductility nor fufibility, must not rank in the number of metals, whose essential and common properties are to be ductile and fusible. Platina, after the examination I gave it, did not appear to be a new metal different from every other, but an alloy of iron and gold formed by Nature, in which the quantity of gold predominated over the iron; and here follow the facts on which I founded this opinion.

Of 8 ounces 35 grains of platina, furnished me by M. de Angivilliers, and which I presented to a strong strong loadstone, remained only i ounce, I dram, 9 grains, all the rest was taken away by the loadstone; so that there remained but 2 drams, which was reduced into powder which stuck to paper, and blackened them, as I shall presently observe. This makes, therefore, nearly fix fevenths of the whole which was attracted by the loadstone, which is so confiderable a quantity, that it is impossible to suppose that iron is not contained in the intimate substance of platina, but that it is even there in a very great quantity. There is more; for if. I had not been weary of these experiments which remained several days, I should have still attracted a great part of the remainder of the 8 ounces by my loadstone; for the loadstone still attracted some grains one by one, and sometimes two. There is, therefore, much iron in platina, and it is not fimply mixed therewith as a foreign matter, but intimately united and making part of its substance; or, if this is denied, it must be supposed, that there exists a second matter in Nature which is attractable by the loadstone, like iron: but this supposition will be overthrown by other circumstances I shall relate.

All the platina I have had an opportunity to examine, has appeared to be mixed with two different matters, the one black, and very attractable by the loadstone; the other in larger grains, of a pale yellow, and much less magnetic than the first. Between these two matters, which are the two extremes of this kind of mixture, is found all the intermediate links, whether with respect to magnetism, colour, or fize of the grains. The most magnetic, which are, at the same time, the blackest and smallest, reduce easily into powder by a very flight friction, and leaves on white paper the fame mark as lead; Seven leaves of paper which were fuccessively made nse of to expose the platina to the action of the loadstone, were blackened over the whole extent occu-VOL. V. pied

pied by the platina; the last lest less than the first. in proportion as it touched it, and as the grains which remained were less black and magnetic: the largest grains, which are the blackest and least magnetic, instead of crumbling into powder like the fmall black grains, are, on the contrary, very hard, and refift all trituration; nevertheless, they are fusceptible of extension in an agate mortar, under the reiterated strokes of a pestle of the same matter, and I flattened and extended many grains to the double or treble extent of their furface: this part of platina has, therefore, a certain degree of malleability and ductility; whereas, the black part appears to be neither malleable nor ductile. The intermediate grains participate of the qualities of the two extremes; they are brittle and hard, they break or extend more difficultly under the strokes of a peftle, and afford a little powder not so black as the first.

Having collected this black powder and the most magnetical grains that the loadstone had the first attracted, I discovered that the whole was iron, but in a different state from common iron. This reduced into powder and filings, contracted moisture, and rusted very readily: in proportion as the rust increased, it grew less magnetic, and absolutely finished by lofing this magnetical quality when it was entirely and intimately rufted; whereas this iron powder, or this feruginous fand found in the platina, is, on the contrary, inaccessible to rust, how long soever it be exposed to the air and humidity: it is also more infusible and much less dissoluble than common iron; but this is iron which appears to me to differ only from common iron by a greater purity. This fand is, in fact, iron divested of all combustible matter and terrene parts found in common iron, and even in steel. It appears endowed and covered with a vitrous varnish which defends it from all alteration. What is very remarkable, is, that this pure iron fand does not exclusively belong to the platina ore; I have found it, although always in a small quantity, in many parts where the iron ore has been dug which confumed in my forges. As I submitted to several trials all the ores which I had, before I determined to work on them for the use of my furnaces, I was furprized to fee, that in some of these, which are all in grains, and some of which is attractable by the loadstone; I nevertheless, found particles of iron fomewhat rounded, and shining, like the filings of iron, and perfectly resembling the ferruginous fand of the platina; they are all as magnetic, all as little fufible, and all as difficultly foluble. Such was the refult of the comparison I made on the sand of platina, and of the fand found in both my iron ores, at the depth of three feet, in earths where water eafily penetrated, I was troubled to conceive from whence these particles of iron could proceed, how they had been defended against rust for the ages that they were exposed to the humidity of the earth, and how this very magnetical iron might have been produced in veins of mines which are not fo at all. I called experience to my aid, and enlightened myself on all these points sufficiently to be satisfied: I knew, by a number of observations, that none of our iron ore in grain are attractable by the loadstone, and was well perfuaded, as I still am, that all iron ores which are magnetical, have acquired this property only by the action of fire: that the mines of the North. which are fo magnetical as to be fought after by the compass, must owe their origin to fire, and only formed by the means, or the intermedium of water. I then thought that this ferruginous and magnetic fand, which I found in a fmall quantity in my iron mines, must owe its origin to fire; and having examined the place, I was confirmed in this idea. This magnetical fand is found in a wood, from time immorial; they made anciently there, and still do make. make coal furnaces. It is likewise more than probable, that there were formerly considerable fires here. Coal and burnt wood produce iron dross, which includes the most fixed part of the iron which vegetables contain: it is this fixed iron which forms the sand here spoken of when the dross is decomposed by the action of the air, sun, and rain: for then these pure iron particles which are not subject to rust, nor to any other kind of alteration, suffer themselves to be carried away by the water, and penetrate into the earth with it at some seet deep, What I here advance may be verified by grinding the dross well burnt: we shall there find a small quantity of this pure iron, which, having resisted the action of the fire, equally resists that of the sol-

vents, and does not ruft at all.

Being satisfied on this head, and after having compared the fand taken from my iron ores and dross with that of the platina, enough not to doubt of their identity, I was not long in imagining, confidering the specific gravity of platina, that if this pure iron fand, proceeding from the decompofition of drofs, instead of being in an iron mine, was found near to a gold one, it might, by uniting with this metal, form an alloy which would be absolutely of the same nature as platina. It is known that gold and iron have a great affinity: it is known that most iron mines contain a small quantity of gold: it is known how to give to gold the tint, the colour, and even the brittleness of iron, by fusing them together. This iron-coloured gold is used on different golden jettels to vary the colours; and this gold mixed with iron is more or less grey, and more or less tempered, according to the quantity of iron which enters the mixture. I have feen it of a tint absolutely like the colour of plating. Having enquired of a goldsmith the proportion of gold and iron in this mixture, which was of the colour of platina, he informed me, that gold of 24 carats was no more than 18; that a fourth part of iron entered therein. It will be perceived, that it is nearly the proportion which is found in the natural platina, if we judge of it by the specifical weight: this gold made with iron is harder and specifically less weighty than pure gold. All these agreements, all these common qualities with platina, have persuaded me, that this pretended metal is, in fact, only an alloy of gold and iron, and not a particular substance, a new and perfect metal different from every other, as chemists have advanced.

It may also be recollected, that the alloy makes all metals brittle, and that when there is a penetration, that is to say, an augmentation in the specific gravity, the alloy is so much the more tempered as the penetration is the greater, and the mixture become the more intimate, as is perceived in the alloy called Bell-metal, although it be composed of two very ductile metals. Now, nothing is more tempered, nor heavier, than platina; that alone ought to make us suppose, that it is only an alloy made by Nature, a mixture of iron and gold, which in part owes its specific gravity to this last metal, and, perhaps, also in a great part to the penetration of the two matters of which it is composed.

Nevertheless, this specific gravity of platina is not so great as our chemists have reported it. As this matter heated alone, and without any addition, is very difficult to reduce into a mass; that we cannot obtain by the fire of a burning mirror only very small masses, and that the hydrostatical experiments made on small volumes, are so defective, that we cannot conclude any thing therefrom. It appears to me, that they are deceived on the estimation of the specific gravity of this mineral. I put some powder of gold in a little quill, which I weighed very exactly:

I put in the fame quill an equal volume of platina, it weighed nearly a tenth less; but this gold powder was much too fine in comparison of the platina. M. Tillet, who joined to a profound knowledge of metals, the rare talent of making experiments with the greatest precision, repeated, by my request, the specific weight of platina compared to pure gold: for this purpose, he, like me, made use of a quill, and cut gold of 24 carats, reduced as much as possible to the fize of the grains of platina; and he found, by eight experiments, that the weight of platina differed from that of pure gold very near a fifteenth; but we have both observed, that the grains of gold had much sharper angles than the platina: all the angles are blunt; it is even foft; whereas the grains of this gold had sharp and cutting angles, fo that they could not adjust themselves, nor heap one on the other as eafily as those of platina; whereas, on the contrary, the gold powder I made use of was fand gold, such as is found in river fand: these adjust themselves much better one against the other. I found about a tenth difference between the fpecifical weight of these and platina: nevertheless, these are not pure gold, and more than two or three carats is often wanted, which must diminish the specifical weight in the fame relation. Thus, every thing well confidered and compared, we have thought that we might maintain the refult of my experiments, and affirm, that platina in grains, and fuch as Nature produces it, is at least an eleventh or twelfth lighter than gold. There is every appearance that this error, on the denfity of platina, proceeds from its not having been weighed in its natural state, but only after it had been reduced into a mass; and as this fusion cannot be made but by the addition of other matters and a very fierce fire, it is no longer pure platina, but a composition in which fusing matters

ters are entered, and from which fire has taken the

lightest parts.

Thus platina, instead of being of an equal, or almost equal denfity to that of pure gold, as has been faid, is only a denfity between that of gold and iron, and only nearer this first metal than the last. Supposing, therefore, that the cube foot of gold weighed 1326lb. and that of iron 280, that of platina in grains will be found to weigh about 1194 lb. which supposes more than 3 of gold to 4 of iron in this alloy, if there is no enetration; but as we extract 6-7ths by the load-Tone, it might be thought, that there is more than iron therein; so much the more as in continuing this experiment, I am perfuaded, that we should be able with a strong loadstone to bring away all the platina, even to the last grain. Nevertheless, we must not conclude that iron is contained therein in fo great a quantity; for when it is mixed by the fusion with gold, the mass which results from this alloy is attractable by the loadstone, although the iron is in no great quantity therein. I faw Mr. Baume have a piece of this alloy weighing 66 grains, in which was only entered 6 grains, that is to fay, \frac{1}{4} of iron, and this button was eafily taken up by the loadstone. Hence the platina might possibly contain only iron, or 16 gold, and yet give the same phænomena; that is to fay, to be attracted entirely by the loadftone; and this perfectly agrees with the specific weight which $\frac{1}{12}$ less than gold.

But what makes me presume, that platina contains more than $\frac{1}{11}$ of iron or $\frac{16}{11}$ of gold, is, that the alloy from this proportion is still of the gold colour, and much yellower than the highest coloured platina, and that more than $\frac{1}{4}$ iron or $\frac{3}{4}$ gold is requisite for the alloy to be precisely of the natural colour of platina. I am, therefore, greatly inclined to think, that there

might possibly be this quantity of $\frac{1}{4}$ iron in platina. We were assured (Mr. Tillet and me), by many experiments, that the sand of this pure iron which contained platina, is heavier than the filings of common iron. Thus, this cause, added to the effect of penetration, is sufficient for the reason of this great quantity of iron contained under the small volume indicated by the specific weight of platina.

On the whole, it is very possible that I am deceived in some of the consequences which I have drawn from my observations on this metallic substance: I have not been able to make so prosound an examination as I could wish; what I say, is only what I have observed, and may perhaps serve for

better inspections.

FIRST ADDITION.

As I was on the point of delivering these leaves for impression, chance led me to tell my ideas of the platina to the Count de Milly, who has a great knowledge in physic and chemistry; he informed me, he was nearly of my opinion; I gave him the Memoir to inspect, and two days after he did me the favour to send me the following observations, which I think as good as mine, and which he has permitted me to publish.

"I exactly weighed thirty-fix grains of platina;" I laid them on a sheet of white paper to be able to

- " observe them better with a loup; I perceived there three different substances: the first had the me-
- " tallie luftre, and was the most abundant: the
- " fecond, drawing a little on the black, pretty well refembled a feruginous metallic matter which
- could undergo a confiderable degree of fire, fuch
- as the fcoria of iron, vulgarly called machefer: the third less abundant than the two first, i. e. sand
 - int, i. e. land

where the yellow, or topaz colour, is the most predominant. Each grain of fand, considered separate, offered to the fight regular chrystals of dif-

ferent colours. I remarked tome in an hexagon form, terminating in pyramids like rock chrystal;

" and this fand feems to be no other than a detritus of

" chrystal, or quartz of different colours.

"I formed a defign of separating as exactly as possible, these different substances by means of the loadstone, and to put aside the part the most attractable by the loadstone, from that which was seless so, and from that which was not so at all; then to examine each substance particularly, and to submit them to different chemical and mechanical heats.

"No. 3. five grains.
"No. 1. examined by the loup, presented only a mixture of metallic parts, a white sand bordering on the greyish, flat and round, or black vitrisorm

" fand, refembling pounded fcoria, in which very rufty parts are perceptible: in short, such as the

" fcoria of iron prefents after having been exposed to

" moisture.

"No. 2. presented nearly the same thing, excepting that the metallic parts predominated, and that there were very sew rusty particles.

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" No. 3. was the fame, but the metallic parts " were more voluminous; they refembled melted " metal which had been thrown into water to be " granulated; they are flat and of all forts of figures, rounded on the corners.

" No. 4. which had not been carried off by the " magnet, but some parts of which still afforded " marks of fenfibility to magnetism; when the mag-

" net was moved under the paper where they were " in, was a mixture of fand, metallic parts, and real

" fcoria, friable between the fingers, and which " blackened in the fame manner as common fcoria.

" The fand feemed to be composed of small rock, "topaz, and cornelian chrystals. I broke fome on " a steel, and the powder was like varnish reduced

" into powder; I did the same to the scoria; it " broke with the greatest facility, and presented a " black powder, which blackened the paper like the

" common.

" The metallic parts of this last (No. 4.) appeared " more ductile under the hammer than those of " No. 1. which made me imagine they contained " less iron than the first: from whence it follows, " that platina may possibly be no more than a mix-" ture of iron and gold made by Nature, or perhaps " by the hands of men, as I shall hereafter take no-" tice.

"I endeavoured to examine, by every possible " means, the nature of platina: to assure myself of

" the prefence of iron in platina by chemical means, " I took No. 1. which was very attractable by the " magnet, and No. 4. which was not; I fprinkled

" them with fuming spirit of nitre; I observed it "then with the microscope, but I perceived no effer-

" vescence: I added distilled water thereon, and it " fill made no motion, but the metallic parts went

" of and acquired a new brilliancy, like filver: I

et let this mixture rest for five or fix minutes, and

having still added water, I threw some drops of alkaline liquor saturated with the colouring matter

of Prussian blue, and a very fine Prussian blue was

" afforded me on the first.

"No. 4. treated in the fame manner, gave the fame refult. There are two things very fingular to remark in these experiments; first, That it passes current among chemists who have treated on the platina, that aquasortis, or spirit of nitre, has no action on it. Yet, as we have just observed, it dissolves it sufficiently, though without efferves cence, to afford Prustian blue, when we add the alkaline liquor phlogisticated and saturated with the colouring matter, which, as is known, preci-

" pitates iron into Prussian blue.
" Platina, which is not sensible to the magnet,

"does not contain less iron, fince spirits of nitre diffolves it enough and without effervescence to make

" Pruffian blue.

"From whence it follows, that this fubstance which the modern chemists, perhaps too greedy of the marvellous, and willing to give something novel, and look upon as a ninth metal, may possibly be, as I have observed, only a mixture of gold and iron.

"Without doubt there still remains many experiments to be made, to determine how this mixture
has not place, if it is the work of Nature, or if it
is the produce of some volcano; or, simply, the
produce of the Spaniards labours in the New World

" to acquire gold in the mines of Peru: I shall here-

" after mention my conjectures thereon.

"If we rub platina on white linen, it blackens it like common fcoria, which made me fuspect that it is the parts of iron reduced into fcoria found in this platina which gives it this colour, and which feem in this state only to have tried the action of a violent fire. Besides, having a fecond time

examined platina with my loup, I perceived there in different globules of running mercury, which made me imagine that platina might be the produce of the hands of man, in the following man-

" ner:

"Platina, as I have been told, is taken out of the oldest mines in Peru, which the Spaniards explored after the conquest of the New World. In those dark times only two methods were known of extracting gold from the sands which contained it; first, by an amelgama with mercury; secondly, by drying it. The golden sand was rubbed with quicksilver, and when that was judged to be loaded with the greatest part of the gold, the sand was thrown away, which was named Crasse, as useless

" and of no value.

"This method was made with very little judg-" ment: to extract it, they began by mineralizing " auriferous metals by means of fulphur, which has " no action on gold, the specific weight of which is er greater than that of other metals; but to facilitate " its precipitation iron was added, which loaded it-" felf with the fuperabundant fulphur, a method " which is still followed. The force of fire vitrifies " one part of the iron, the other combines itself with a small portion of the gold, and even of filver which " mixes with the fcoria, from whence it cannot be " drawn but by ftrong fufions, and without being " well instructed in the suitable intermediums which " the docimafifts make use of. Chemistry, which is " now arrived to great perfection, affords, in fact, " means to extract the greatest part of this gold and " filver: but at the time when the Spaniards explored " the mines of Peru, they were, doubtless, ignorant " of the art of mining with the greatest profit: be-" fides, they had fuch great riches at their difpofal, " that they, probably, neglected the means which " would have cost them trouble, care, and time: " therefore, there is every appearance that they " contented themselves with a first fusion, and threw " away the fcoria as ufelefs, as well as the fand which " had escaped the quickfilver, and perhaps they made " only a heap of thefe two mixtures, which they

" regarded as of no value.

"Their scoria contained gold and much iron under " different states, and that in different proportions " unknown to us, but which, perhaps, are those " which gave origin to the platina. The globules of " quickfilver, which I have observed, and those of se gold, which I have diffinctly feen with the affift-" ance of a good loup, in the platina I had in my " hands, have given birth to the ideas which I have " written on the origin of this mineral; but I only " give them as hazardous conjectures, to acquire fome " certainty it must be precisely where the platina mines are fituated; if they have been anciently " explored, whether it is extracted from a new foil, or " if they are only rubbish, and to what depth they are " found; and laftly, if the hands of man is expressed " there or not; all which might affift to verify or " deftroy the conjectures I have advanced."

MARKS.

THESE observations confirm mine in almost every point. Nature is the fame, and prefents herfelf always the fame to those who know how to observe her: thus, we must not be surprized, that without any communication M. de Milly has obferved the fame things as I did, and that he has deduced the fame confequence therefrom; that platina is not a new metal different from every other, but a mixture of iron and gold. To reconcile his observations fill more with mine, and to enlighten, at the fame time, the doubts which remain in great numbers bers on the origin and formation of platina, I have thought it necessary to add the following remarks.

1. M. de Milly diffinguishes three kinds of matters in platina; to wit, two metallic; and the third, non-metallic of a quartzeuze and chrystalline substance and form. He has observed, as well as me, that of the two metallic matters, the one is very attractable by the magnet, and that the other is very little, or not at all. I have made mention of thefe two matters as well as him, but I have not spoken of the third, which is not metallic, because there is none at all there, or very little in the platina on which I have made my observations. There is an appearance, that the platina which M. de Milly made use of was not so pure as mine, which I have observed with care, and in which I have feen only fome fmall transparent globules like white melted glafs, which were united to the particles of platina or feruginous fand, and which are carried any where by the magnet. These transparent globules were very few, and in eight ounces of platina which I narrowly inspected, and others infpected with a very firong loup, we have never perceived regular chrystals. It has appeared to me, on the contrary, that all the transparent particles were globulous like melted glass, and all attached to metallic parts: nevertheless, as I do not at all doubt of the veracity of M. de Milly's observation, who obferved quartzeuze and chrystalline particles of a regular form, and in a great number, in his platina, I thought I ought not to confine myself to the examination of the platina alone, of which I have heretofore fpoken of. I found fome in the king's cabinet, which Lexamined with M. Daubenton, of the Academy of Sciences, and which appeared to us much less pure than the first; and we, in fact, remarked a great number of small prismatic and transparent chrystals, fome of a ruby colour, others of a topaz, and others perfectly white; therefore the Count de Milly was

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not deceived in his observations; but this only proves that there are some mines of platina much purer than others, and that in those which are the most so, none of these foreign bodies are found. M. Daubenton has also remarked some grains flat at bottom and rough at top, like melted metal cooled on a plain. I have very distinctly seen one of these hemispherical grains, which might indicate that platina is a matter which has been melted by the fire, but it is very fingular, that in this matter melted by the fire, fmall chrystals, topaz, and rubies are found, and I do not know whether we ought not to suspect fraud on the side of those who supplied this platina, and which to increase the quantity. have mixed it with these chrystalline sands, for I repeat it, that I have never met with these chrystals in more than one half pound of platina, given me by the count of Angilliviers.

2. I have found, as well as M. de Milly, gold fand in Platina; it is readily discovered by their colour, and because it is not magnetical, but I own, that I have never perceived the globules of mercury, which M. de Milly did. I will not from thence deny their existence, only it appears that the sand of gold meeting with the globules of mercury in the same matter, they might be soon amalgamated, and might not retain the yellow colour of gold which I have remarked in all the gold sand that I could find in half a pound of platina; besides, the transparent globules which I have just spoken of, resemble greatly the globules of live and shining mercury, insomuch that at the first glance it is easy to be

deceived therein.

3. There were much fewer tarnished and rusty parts in my first platina, than in that of M. de Milly, and it is not properly rust which covers the surface of those feruginous particles, but a black substance produced by fire and perfectly similar to that which

covers the furface of burnt iron. But my fecond platina, that is to fay, that which I took from the royal cabinet, had also in common with that of the Count de Milly, a mixture of some feruginous parts, which under the hammer, is reduced into a yellow powder, and had all the characters of ruft. fore this platina of the royal cabinet, and that of M. de Milly, resembling in every respect, it is probable that they proceeded from the same part, and by the same road. I even suspect that both had been fophisticated and mixed above half, with foreign chrystilline and furuginous rusty matters,

which is not met with in the natural platina.

4. The production of Pruffian blue by platina, appears evidently to prove the presence of iron in the same parts of this mineral, which is the least attractable by the magnet, and at the fame time confirms what I have advanced on the intimate mixture of iron in its fubstance. The flowing of platina by spirits of nitre, proves that although it has no sensible effervescence, this acid attracts the platina in an evident manner; and the authors, who have afferted the contrary, have followed their common track, which confifts on looking on all actions as null which does not produce an effervescence. These fecond experiments of M. de Milly, would appear to me very important, if they fucceeded always alike.

5. In fact, many necessary matters are wanting to us, to pronounce affirmatively on the origin of platina. We know nothing of the natural history of this mineral, and we cannot too greatly exhort those who are inclined to examine it on the fpot, to make us acquainted with their observations. In expectation of which, we are forced to confine ourselves to conjectures, some of which appear only more probable than others. For example, I do not imagine platina to be the work of man. The Mexicans and

Peruvians knew how to force and work gold before the arrival of the Spaniards, and they were not acquainted with iron, which nevertheless they must have employed in a great quantity. The Spaniards themselves did not establish furnaces to suse iron in this country, when they sirst inhabited it. There is, therefore, all appearances to think, that they did not make use of the filings of iron, for the seperation of gold. At least in the beginning of their labours, which does not go above two centuries and a half back, a time much too short for so plentiful a production as platina, which is found in large quanti-

ties in many places.

Befides, when gold is mixed with iron, by fufing them together, we may always by chemistry seperate them and extract the gold: whereas that, hitherto chemists have not been able to make this separation in platina, nor determine the quantity of gold contained in this mineral. This feems to prove, that gold is united therewith in a more intimate manner than the common alloy, and that iron is also therein, as I have observed, in a different state from that of common iron. Platina, therefore, appears to me not to be the work of man, but the production of nature, and, I am greatly inclined to think, that it owes its first origin to the fire of Vulcanos. Burnt iron, intimately united with gold by fublimation or fusion, may have produced this mineral, which having been at first formed by the action of the fiercest fire, will afterwards have felt the impressions of water and the reiterated frictions, which have given it the form they give to every other body, that is to fay, that of blunt angles. But water alone might have produced the platina; for supposing gold and iron divided as much as possible by the humid mode, their molecules, by uniting, will have formed the grains which compose it, and which from the heaviest to the lightest contain gold and iron; the proposition VOL. V.

of the chemist who offers to render nearly as much gold as they shall furnish him with platina, seems to indicate, that there in fact, is only in of iron to if of gold in this mineral, or possibly less. But the nearly of this chemist is probably a fifth or a fourth, and indeed if he could realise his promise to a fourth, it would be a great deal.

SECOND ADDITION.

DEING at Dijon the fummer of 1773, the Acade-Dmy of Sciences and Belles Lettres there, of which I have the honour to be a member, seemed desirous of hearing the lectures of my observations on platina. I complied fo much the more readily, as it was on a fubject so fresh, too much information nor consultation could not be had thereon, and as I had room to hope to derive fome lights from a company where many well instructed persons met. M. de Morteau, advocate-general to the parliament of Burgundy, as great a phyfician as lawyer, took the resolution to work on the platina; I gave him a portion of that which I had attracted by the loadstone, and a portion of that which I had found infenfible to magnetism, requesting him to expose this fingular mineral to the strongest fire that he could possibly make; and fome time after, he fent me the following experiments, which he was pleased to subjoin to mine.

EXPERIMENTS made by M. DE MORTEAU, September 1773.

"MONSIEUR the Count de Buffon, in a jourmey to Dijon, the summer of 1773, having caused me to remark in half a dram of platina, which M. de Baume had sent him in 1768, grains in form of buttons, others flatter, and some black " and fealy; and having separated by the load-stone, " those which were attractable from those which appeared not fo, I tried to form Prussian blue with " both. I sprinkled the fuming nitrous acid on the non attractable parts, which weighed 21 grains. " Six hours after, I put diffilled water on the acid, " and sprinkled alkaline liquor, saturated with a " colouring matter; there was not a fingle atom of " blue, the platina had only a little more bright-" nefs. I alike sprinkled the fuming acid on the 33 grains and an half of remaining platina, part of which was attractable, the same Prussian alkali " precipitated a blue feculency, which covered the bottom of a pretty large bason. The platina af-" ter this operation, was greatly run off like the " first: I washed it and dried it, and I found it had " not lost 4 of a grain, or 1 ; having examined it " in this state, I perceived there a grain of beauti-" ful yellow, which was a grain of gold. " M. de Fourcy had newly published, that the " diffolution of gold was thrown down in in a blue " precipitate by the Prussian alkali, and had placed " this circumstance in a table of affinity; I was

"tempted to repeat this experiment, and sprinkled, in consequence thereof, the phlogisticated alkaline liquor in the dissolution of gold, but the colour of this dissolution did not change, which made me suspect that the dissolution of gold made use of by M. de Fourcy, might possibly have not been

" fo pure.

"At the same time, the Count de Buffon having given me a sufficient quantity of platina to make further essays, I undertook to separate it from all foreign bodies by a good font. Here follows the

" processes and the results I met with."

FIRST EXPERIMENT.

" Having put a dram of platina into a cupel, in a furnace, spoken of by M. Macquer, in the Memoirs of the Academy of Sciences, 17.58, "I kept up the fire two hours, the covers funk down, the supporters having run, neverthese lefs, the platina was found only agglutinated; it fluck to the cupel, and had left spots of a rusty colour; the platina was then tarnished even a little black, and had only augmented a se grain of weight; a quantity very weak in compa-" rison with that which other chemists have observed: " what furprized me still more was, that this dram of platina, as well as all that is used for other exse periments, had been successively carried away by the loadstone and made a portion of of eight " ounces of which Mr. Buffon has before spoken " of."

SECOND EXPERIMENT.

"Half a dram of the same platina, exposed to the same fire in a cupel, was also agglutinated; it adhered to the cupel, on which it had left spots of a rusty colour, the augmentation of weight was found to be nearly in the same proportion, and the surface as black."

THIRD EXPERIMENT.

"I put this half dram into a new cupel, but in"flead of the cover, I placed a leaden crucible.

"This I kept in the most extreme heat for four
hours: when it was cooled, I found the crucible
foldered to the support, and having broken it, I

perceived that nothing had penetrated into the internal part of the crucible, which appeared to be
only

ff only more gloffy than before. The cupel had " preserved its form and position, it was a little 55 cracked, but not enough to admit of any pene-" tration; the platina was also not adherent to it, " though agglutinated, but in a much more inti-" mate manner than at first, the grains were less " angular, the colour clearer, and the brillancy " more metallic: but what was most remarkable " during the operation there iffued from its " furface, probably in the first moments of its " refrigeration, three drops of water, one of which that rose perfectly spherical, was carried up on a " small pedicle of the vitreous and transparent mat-" ter. It was of an uniform colour, with a flight " tint of red, which did not deprive it of any tran-" fparency. The two other drops of glass, the " fmallest of which had likewise a pedicle, and the other none, but was only attached to the platina " by its external furface."

FOURTH EXPERIMENT.

" I endeavoured to affay the platina, and for that intent put a dram of the like grains taken up by "the loadstone into a cupel, with two drams of " lead. After having kept up a very strong fire for " two hours, I found an adherent button, covered with a yellowish and spungeous crust of two drams "twelve grains weight, which announces that the so platina had retained one dram twelve grains of " lead.

" I put this button into another cupel in the " fame furnace, observing to turn it: it only lost " twelve grains in two hours; its colour and form

" were very little changed.

"The fame piece of platina being put into " Macquer's furnace, and a fire kept up for three 66 hours, when I was obliged to take it out, because the "the bricks entirely run; the platina was become more metallic; it, nevertheless, adhered to the cupel; and it lost this time thirty-four grains. I threw them into the fuming nitrous acid to assay it; there arose a little effervescence when I added distilled water thereon; the platina lost two grains, and I remarked some small holes, like those which

" its flying off might occasion,

"There then remained only twenty-two grains of lead in the platina. I began to form a hope of vitrifying this remaining portion of lead, for which purpose I put the same piece of platina into a new cupel, and by the care I took for the admission of air and other forementioned precautions, the activity of the sire was so greatly augmented, that it required a supply every eleven minutes, and kept it to that degree for sour hours, and then permitted it to cool.

"I perceived the next morning that the leaden crucible had refifted, and that the supporters were only glazed by the cinders. I found a piece in the cupel, no way adherent, of a uniform colour, approaching more the colour of tin than any other metal, but only a little ragged. It weighed ex-

" actly one dram.

"All therefore announced, that this platina had endured a perfect fusion; that it was perfectly pure; for suppose it still contained lead, we must also suppose that this mineral had exactly lost as much of its own substance as it had of foreign matter, and such a precision cannot be the effect

of pure chance.

"I passed several days with M. de Busson, whose company has the same charms as his style, and whose conversation is as complete as his books; I took a pleasure in presenting him with the productions of my essay, and examined them with him.

"I. We observed that the dram of platina, agglutinated by these experiments, was not at-

" tracted by the loadstone; that, nevertheless, the magnetical bar had an action on the grain that

" were loofened from it.

"2. The half dram of the third experiment was not only attractable in mass, but the grains of gold feparated therefrom, did not themselves give any fign of magnetism.

"3. The platina of the fourth experiment was

" also absolutely insensible to the loadstone.

"4. The specific weight of this piece was determined by a good hydrostatical balance, and for
the greater certainty, compared to coined gold,
and to other very pure gold, used by M. Busson
in his experiments, their density was found as follow, with water in which they were plunged.

Pure gold - 19 1 Coin gold - 17 1 Platina - 14

" 5. This piece of platina was put upon steel to " try its ductibility; it supported the hammer very well for a few strokes, its surface became flat and " even, a little fmooth in the parts which were " ftruck, but it split soon after, and a portion sepa-" rated from it, making nearly a fixth part of the " whole. The fracture presented many cavities, " fome of which had the whiteness and brillancy of " filver, in others we remarked several points like " christallisation; the tops of these points seen " with the loup, was a globule absolutely fimilar " to that of the third experiment. As for the reft, all the parts of this piece of platina were compact, " the grain finer and closer than the best brass, to " which it refembled in colour. Whatever pieces " we offered to the loadstone, not one was attracted " thereby, but having powdered them again in an agate mortar, we remarked that the magnetical bar raised up some of the smallest every time they

" were placed under it.

"This new appearance of magnetism was so much the more surprising as the grains were detached from the agglutinated mass of the second experiment, which appeared to us to have lost all fensibility at the approach and contact of the loads frome. We again in consequence of that, took fome of these grains which were alike powdered, and soon perceived the smallest parts sensibly attach themselves to the magnetic bar. It is impossible to attribute this effect to the smoothness of the bar, nor to any other cause foreign to magnetism. A piece of smooth iron, applied in the fame manner on the parts of this platina, never

" raised up a fingle one.

" By the recital of these experiments, and the ob-" fervations which have arisen therefrom, we may " judge of the difficulty of determining the nature of platina; it is very certain that it contains " fome vitrifiable parts, vetrifiably even without the addition of a fierce fire; it is very certain that all platina contains iron and attractable parts, but if the Pruffian alkali never affords blue but with the grains which the loadstone attracts, we should conclude, that those which refist it are pure pla-" tina, which of itself has no magnetical virtue, and which iron does not make an effential part, We must hope that a sufficient susion, or perfect " cupellation might decide this question; at least, " all announces that these operations had, in fact, deprived it of every magnetical virtue by separat-" ing it from all foreign bodies; but, the last observation proves in an invincible manner, that this " magnetical property was, in reality only weakened " there, and perhaps masked or buried, since it has " re-appeared when it has been ground.

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FROM these experiments there results, r. That we may expect platina to be melted without addition, by applying the fire to it several times successively, because the best crucibles might not resist the action of so fierce a fire during the whole time that the complete operation requires.

2. That by melting it with lead, and affaying them feveral times, we should in the end vitrify all the lead and the platina, that this experiment would be able to purge it from a part of the foreign matters it

contains.

3. That by melting it without any addition, it feems to purge itself partly into the vitrescible matters it includes, since it emits to its surface small drops of glass which form pretty considerable masses, and that

we can eafily feparate them after refrigeration.

4. That by making experiments on Prussian blue with the grains of platina, which appeared to be most insensible to the loadstone, we were not always certain of obtaining it, as that circumstance never fails with grains which have more or less sensibility to magnetism; but, as M. Morveau made this experiment on a very small quantity of platina, he proposes to

repeat it.

5. It appears that neither fusion nor cupellation can destroy all the iron with which platina is intimately penetrated: the pieces melted or assayed, appeared in reality equally as sensible to the action of the load-stone; but, having pounded them in a mortar, we found magnetical parts; so much the more abundant as the platina was reduced into a finer powder. The first piece, whose grains were only agglutinated, being ground, rendered many more magnetical parts than the second and third, the grains of which had undergone a stronger susion; but, nevertheless, being Vol. V.

both ground, they furnished magnetical parts; insomuch, that it cannot be doubted that there is iron in platina, after it has undergone the fiercest efforts of fire, and the devouring actions of the heat in the cupel. This demonstrates, that this mineral is really an intimate mixture of gold and iron, which hitherto art

has not been able to separate.

6. I and Mr. Morveau made another observation on melted and afterwards on ground platina, which is, that it takes in grinding precifely the fame form as it had before it had been melted: all the grains of this melted and ground platina are fimilar to these natural platinas, as well for the form as for the variety of the fize; and they appear to differ only because the smallest, alone suffer themselves to be raised. by the loadstone, and in fo much the less quantity as the platina has endured the fire. This feems also to prove, that, although the fire has been firong enough not only to burn and vitrify, but even to drive off a part of the iron with other vitrescible matter which it contains; the fusion, nevertheless, is not so complete as that of other perfect metals, fince in grinding it retakes the fame figure as it had before fusion.

FOURTH MEMOIR.

Experiments on the Tenacity and Decomposition of Iron.

In the first Memoir we have seen, that iron loses weight every time it is heated by a sierce sire, and that the bullets heated white three times, lost the twelfth part of their weight: we might be directly led to think, that this loss must not be attributed only to the diminution of the volume, by the scoria which falls off from the surface in little scales, but we consider that the small bullets, of which the sur-

face is consequently greater, relatively to the volume, than that of the large, lose less; and that the large bullets lofe proportionably more than the fmall: we shall perceive, that the total loss of weight must not be simply attributed to the fall of the scales, but also to an internal alteration of all the parts of the mass which the fierce fire diminished. and rendered fo much the lighter as it was applied the oftener and longer : and, in fact, if we collect the scales which fall off, we shall find, that on a bullet which, for example, has loft eight ounces by a first heat, there will only be one ounce of these scales, and that all the rest of the loss can only be attributed to this internal alteration of the substance of iron which loses its density each time that it heats; infomuch, that if we should repeat the operation often, the iron would be reduced to no more than a fusible and light matter, of which no use could be made; for I have remarked, that the bullets not only loft their weight; that is to fay, their denfity, but that at the same time they had loft much of their folidity; that is to fay, of their quality, on which depends the coherence of the parts; for I have obferved, by striking them, that they could be broken easier the oftener and longer they had been heated.

It is, without doubt, because it was not known to what point this alteration of iron extends, or rather, because it was not doubted, that for several years bullets have been heated in our artillery. Now, I am assured, that the caliber of cannons newly cast, being narrower than that of the ancient cannons, the bullets required diminishing: to attain this they were heated white, that they might be the easier scraped afterwards in turning; and that often they were obliged to heat them five, fix, eight, and even nine times, to reduce them as much as was necessary. Now, it is evident, by my experiments, that this practice is bad; for a bullet thus heated nine times.

times, must lose 4 of its weight, and perhaps 3 of its folidity: becoming also brittle, it cannot serve for the purpose of making a breach; and becoming lighter, it has also the great disadvantage of not go-

ing so far as the rest.

In general, if we would preferve in iron its folidity and nerve; that is, its mass and force, we must not expose it to a fire oftener nor longer than is necessary; it will fuffice, for most uses, to redden it without forcing the fire to heat it white; this last degree of heat will never fail to waste it; and in works where it imports to preferve all its nerve, as in the bends forged for cannons, they should, if possible, not be heated above once, that they might be beat, bent, and foldered by one operation; for when iron has acquired under the hammer all the force it is fusceptible of, the fire can do no more than diminish it. But it is for artists to see to what point this metal must be wrought to acquire its whole force, and which would not be impossible to be determined by experiments.

By experiments it will be found how advantageous it is to use only strong iron in buildings, and in the construction of vessels \(\frac{3}{2}\) the less is required, and we

shall have befide I more folidity.

By the like experiments, and by making rods of iron of different fizes malleable once, twice, and thrice, we might ascertain the maximum of the force of iron: to combine in a certain manner the lightness of arms with their folidity, to manage the matter in other works without the danger of a rupture, and to work this metal on uniform and constant principles. These experiments are the only means of perfectionating the art of manipulation of iron; the state would derive great advantages therefrom, for it must not be thought that the quality of iron depends on that of the ore: that, for example, English, or German, or Swedish iron, is better than that of France;

France; that the iron of Berri is softer than that of Burgundy; the nature of mines, is of no effect therein: it is the manner of heating them which does all; and what I can affert, from having seen it myself, that is, by malleating iron much, and heating it but little, we give more strength to it, and approach this maximum, of which I can only recommend the enquiry, and to which we can attain by the experi-

ments I have indicated.

In bullets which I feveral times submitted to the trial of the greatest fire; I perceived that the iron lost as much more of its weight and strength as it was oftener and longer heated; its substance decomposes, its quality alters, and at last it degenerates into a kind of machefer, or porous light matter which reduces into a kind of lime by the violence and long application of fire. Common machefer is of another kind, and though it is vulgarly thought not to proceed, and even cannot proceed only from iron, I have proved the contrary. Machefer is, in fact, a matter produced by the fire; but to form it, it is not necessary to use iron, nor any other metal: with wood and coal burnt and impelled by a strong fire, we shall obtain machefer in a very great quantity. And, if it is pretended, that this machefer proceeds only from the iron contained in the wood (because all vegetables contain it in a greater or leffer quantity), I ask why it cannot be extracted from iron in a greater quantity than from wood, whose substance is so different from iron? Since this circumstance is known to me by experience, it supplied me with the knowledge of another which till then appeared to me inexplicable. In high grounds, and especially in forests where there is neither rivers nor brooks, and where, consequently, there never were any forges, no more than any fign of a volcano or subterraneous fire, we find often great lumps of machefer which two men can scarcely lift. I saw them for the first time time in 1745, at Montigny l'Encoupe, in the forests of M. de Trudaine; I searched after it and sound some since in our woods of Burgundy, which are still more remote from the water than those of Montigny, and it has been sound also in several places: the small pieces have appeared to me to proceed from some coal surnaces which have been suffered to burn, but the large can only be caused by some sire in the forest when it was in sull glory, and the trees were large and near enough to make a very sierce sire,

and to feed it for a long time.

Machefer, which may be looked upon as a refidue of the combustion of wood, contains iron; and we shall find in another Memoir the experiments I have made to discover, by this refidue, the quantity of iron which enters into the composition of vegetables. This dead earth into which iron is reduced by the too long action of the fire, has not appeared to me to contain more iron than the machefer of wood. which feems to prove, that iron is, like wood, a combustible matter, which iron can equally devour by applying it only more violently and a longer time. Pliny, with great reason says, Ferrum accensum bigni, nift duretur ictibus, corumpitur .- Hift. Nat. lib. xxxiv. cap. xv. We shall be persuaded, if we observe the first loup that is drawn from the fow of iron, this loup is a piece of iron melted a fecond time, and which has not been forged; that is to fay, confolidated by the hammer. When it is drawn from the stove where it underwent the most violent fire, it is heated to a whiteness; it throws off not only sparks, but really burns with a very brisk flame, which would consume a part of its substance if this loup remained too long under the hammer: this iron, we may fay, would be destroyed before it was formed; it would undergo the complete heat of combustion if the stroke of the hammer, by approaching again its part too much divided by the fire.

fire, did not make them take the first degree of tenacity. We draw iron in this state, and still red from under the hammer, it is carried to the refiner's furnace, where it is penetrated with a new fire: when it is white, it is transported in the same manner, and as quick as possible to the hammer, under which it consolidates and extends much more than at first: in short, we replace still this piece to the fire, and it is brought to the hammer under which it is entirely finished.

It is in this manner all common iron is wrought; they receive but two or three strokes with the hammer: thus, they have not near the tenacity they would acquire if they were wrought not fo precipitately. The force of the hammer not only compresses the parts of the iron too much divided by the fire, but by approaching them, it drives off all the foreign matters, and purifies it by consolidating it. The waste of iron in dross is commonly about one third, the greatest part of which is burnt, and the rest flows in fusion, and forms what is called Les Crasses du fer; these are heavier than the macheser of wood, and contains a great quantity of iron, which is, in fact, very impure and rough, but from which we can, nevertheless, extract a part by mixing these crasses ground in a small quantity with the ore which is thrown into the furnace: I have experienced, that by mixing one fixth of these crasses with five fixths of depurated by my fieves, the fusion did not fenfibly change the quality; but if more was put in, it became more brittle, without changing colour or grain: but if the ores are less depurated, these crasses absolutely spoil the fusion; because, being already very rough and brittle of itself, it becomes still more so by the addition of bad matter; infomuch, that this practice which may become useful in the hands of an able master, will in other hands hands produce such bad effects, that the iron pro-

ceeding from it cannot be made use of.

There is notwithstanding means, I do not say of changing, but of correcting a little the bad quality of the fusion, and fosten the sharpness of the iron which proceeds from it. The first of these theans is to diminish the force of the wind, whether by changing the inclination of the tunnel, or by flackening the motion of the bellows; for the more we force the fire, the rougher the iron becomes? The fecond means, and which is still more efficacious; is to throw on the iron which separates from the drofs, a certain quantity of calcareous gravel, or even of lime: this lime serves as a fuser to the vitrifiable parts which the iron contains in too great a quantity, and purges it of its impurities. But these are trifling resources, which we must not admit if we have any other, which would never happen if my processes were followed.

When refiners labour for value, and are paid by the quantity, they, like the founders, make as much iron in the week as they can; they contract their furnace in the most advantageous manner; they press the fire, find that the bellows does not afford sufficient wind, and commonly make as much in two heats as would at least require three: we shall, therefore, never be certain of having iron of a good and like quality, but by paying workmen by the month, and making them break at the end of every week some bars of iron that they deliver, to discover whether they are not too much forced or ne-

glected.

Good casting is, in fact, the base of all good iron, but it often happens, that by bad practice this good iron is spoiled. One of these bad practices, which destroys the nerve and tenacity of iron, is the use most workmen have of dipping into the water the first piece they are at work upon, in order to ma-

nage it, and be able to hold it again the more readily; I have feen, with fome furprize, the prodigious
difference this dipping occasions, especially in the
winter, it not only renders the best iron brittle,
but even changes the grain, and destroys the nerve
so much that we would not imagine it to be the same
iron, if we were not convinced by breaking the
other end of the same bar, which having not been
dipped, retains its nerve and its common grain.
This dipping does much less hurt in summer, but
does always a little; and if we would have iron always of the same good quality, this custom must be
absolutely proscribed, the hot iron must never be
dipped in water, but wait till it is cooled in the air.

The cast ought to be very good to produce iron as nervous and tenacious as that made from remelted old iron, not by throwing it into the fufing furnace, but by putting it into the refiner's fire. Every year a great quantity is bought for my forges, from which, with a little care, an excellent iron is made. But there is a choice to be made, those which proceed from the clippings, or broken pieces of iron wire, called Riblons, are the best, because they are of a purer iron than the rest: they bear likewise a larger price; but, in general, this old iron, although of a middling quality, produces very good, when it is handled by those who know how to treat it: it must never be mixed with the cast; if even there is found some pieces among it, they must be separated: a certain quantity of dross must also be put in the furnace, and the fire less agitated, and not fo fierce as for the other iron, without which a great quantity of it would be burnt, which when heated and of a good quality, only affords one fifth wafte, and does not retain near fo many particles of iron as the rest. With the riblons sent from the wireworkers, who fupply my forges, and from the iron plates which I have fabricated, I have often made iron which was all nerve, and the waste of which VOL. V.

was only one fixth; whereas, the waste of pig iron was commonly double; that is to say, one third, and

often more, to obtain iron of a better quality.

M. de Montbeillard, lieutenant colonel of the royal regiment of artillery, having for many years had the inspection of the manufactures of arms at Charleville, Maubeuge, and St. Stephen, has communicated a Memoir to me, which he presented to the minister, and in which he treats of this fabrication of old iron. He fays, with great reason, "That those pieces which have much surface, and so those which proceed from old iron and horses se shoes, or from rings and buckles, and all pieces " which suppose that the iron made use of " to fabricate them, was supple, must be preferred of for the fabrication of guns." In this Memoir we also find some excellent reflections on the means of perfectionating fire-arms, and to ascertain the refistance by the choice of good iron and the manner of treating it. The author relates a very good experiment, which clearly proves, that old iron, and even the exfoliations, which many take for the fcoria, folder together in the most intimate manner; and that, consequently, the iron which proceeds from it is as good, and perhaps better than any other. But, at the same time, he will agree with me; and he obferves, in the course of his Memoir, that this excellent iron must not be used alone, for the reason even of its being too perfect. In fact, iron which has all its perfection, is only excellent to be used for works that require but a gentle heat; for all brisk heat unnaturalizes it. I have made reiterated trials on pieces of all fizes; the finall unnaturalizes fooner than the large, but both lose the greatest part of their nerve as foon as heated white. A fecond heat, like the first, changes and completes the destruction of the nerve; it even alters the quality of the grain, which from fine, becomes coarle and shining, like

that of the most common iron. A third heat renders these grains still coarser, and suffers the black parts of burnt matter to be seen already between their intervals. At length, by continuing to heat them, we attain the last degree of its decomposition, and reduce them into a black earth, which no longer appears to contain metallic substance: for this black earth has not, like most other metallic dross, the property of revivifying by the application of combustible matters. It contains no more iron than the common macheser extracted from the coal of vegetables; whereas, the dross of other metals revivises almost entirely, at least, the greatest part of it; and this fully demonstrates, that iron is almost entirely combustible.

This iron which is extracted, as much from this earth or iron drofs, as from the machefer produced from coal, has appeared to me of a fingular quality, and is very magnetic and infufible. I have found black fand as magnetic, indiffoluble, and almost infufible, in some of the mines I had explored. This feruginous and magnetic fand is found mixed with that of ore which is not at all fo, and certainly proceeds from quite another cause. Fire produced this magnetical fand, and water the ore; and when by chance they are found mixed, it is from a great mass of wood having been burnt, or furnaces of coal having been made on the foil which incloses the mine; and that this feruginous fand, which is only the waste of the machefer, which water can neither rust nor dissolve, has penetrated by the filtration of the waters near the beds of the ore in grain, which are often only two or three feet deep. It has been observed in the preceding Memoir, that this feruginous fand which proceeds from the machefer of vegetables, and from iron burnt as much as it could be, seems to be the same in every respect as that found in platina. The

The most perfect iron is that which has almost no grain, and which is entirely of an ash colour. iron is very good, and, perhaps, preferable to the first for all uses where this metal is required to be heated more than once before it is used. Iron of the third quality, and which is half nerve and half grain, is the best iron for commerce, because it can be heated two or three times before it is unnaturalized, Iron without any nerve, but with a fine grain, ferves also for many uses; but iron without nerve and large grain, ought to be proscribed, and does the greatest mischief in society, because, unfortunately it is an hundred times more common than the rest: one glance is only requifite for a man versed in the art to know the good or bad quality of iron; but people who make use of it, either in structures or equipages, are not acquainted with it, or do not regard it, and often pay as much as for the best; for iron, which a weight will break, or which rust, will destroy in a little time,

Forasmuch as a brisk heat wastes iron, so much a gentle heat, which only reddens it, feems to ameliorate it. It is for this reason, that iron destined for the battery, does not require fabricating with fo much care as those called fers marchands, or mercantile iron, which ought to have all their quality. Iron makes a separate class; it cannot be too pure, if it contains heterrogenous parts it would become very brittle. Now, there is no other mode of rendering it pure, but by heating it the first time to a whiteness, and hammering it with as much force as precaution, and afterwards heating it again in order to finish depurating it under the hammer, by lengthening it to make iron rods. But iron, destined to be cut to make common flat iron; in one word, all kinds that must pass under cylinders, do not require the same degree of perfection, because they ameliorate in the furnace furnace, where only wood is made use of, and in which all these irons only take the second degree of heat, of a red fire colour, which is fufficient to foften it, and fuffers them to flatten and extend under cylinders, and to be afterwards cut by the workers of iron: nevertheless, if we would have very foft rod iron like that necessary for horse-nails: if we would have iron flatted which has much nerve. as those must be which are used for wheels, and particularly those which are made from a fingle piece. in which at least a third of nerve, or spring, is required, the iron ought to be of a good quality; for, I have observed, that the gentle fire of a stove. and the strong compression of the cylinders, renders, in fact, the grain of the iron much finer, and gives even strength to that which had only a very fine grain; but it never strengthens the large grain of common iron: fo that with bad iron with large grains. rods and flatted iron might be made, whose grain would not be so coarse, but which would be too brittle to be used in the cases I speak of.

It is the same with steel; we cannot have too good stuff to make it of, and it is provoking. that they make use of quite the contrary, for almost all the plates in France are made with common iron; they break in bending, and burn or perish in a very little time; whereas, the plates made in Sweden or England, with good nervous iron, may be bent without breaking, and will endure twenty times longer than the other. At my forges all fizes and all thicknesses have been made; they have been used at Paris for pots and other kitchen furniture. which they had reason to prefer to those made of copper. With this same plate a great number of stoves, chains, and pipes have been made, and I have had the experiment a thousand times reiterated within these four years; that it can endure, as I say, either in the fire or air much longer than the common; but, as it is somewhat dearer, the use of it is less, and it is only asked for in certain uses where the other could not be employed. It may be said, that in France a general compact has been made to make use of only what is the worst of this kind.

With nervous iron we can always make excellent plates: those who flatten these under the hammer, after having heated them in a coal fire, have a very bad custom. Coal fire, impelled by the bellows, spoils this iron; that of the kiln only perfectionates it: befides, it costs as little again to work it by the cylinder as by the hammer. Here interest agrees with the theory of art; it is, therefore, only ignorance which can uphold this practice, which, nevertheless, is the most general; for there is, perhaps, among all the iron plate fabricated in France more than three fourths which have been made by the hammer. This cannot be otherwise, it will be faid, all fmiths have not a refinery by their fides; I acknowledge it, and it is that I complain of. We are wrong in permitting these little particular establish. ments which subfift only by buying iron at the large forges at the best price, to fabricate it afterwards into plates and small iron-work of the worst quality.

Another very important object is plough-iron; it cannot be imagined how great trouble the bad quality of iron gives to the workers of it. Iron is inhumanly put into their hands which breaks at the least stroke, and which they are obliged to renew almost as often as their tillage: they are made to pay a very high price for bad steel, with which the points of this still worse iron is armed, and the whole is lost at the expiration of a year, and often in a shorter time: whereas, by using for these plough-shares, the best and most nervous iron, they might be warranted for twenty years, and even without steeling of the point; for I have made many hundreds of these plough-irons, of which

which I have tried some without steeling, and they are found to be of a stuff sufficiently firm to resist labour. I have made the same experiment on a great number of pick-axes; it is the bad quality of our iron which has established among the iron-workers the general use of putting steel to these instruments, which there would be no need of if they were of good iron sabricated with iron passed under cylinders.

I own that there are certain customs, for which rough iron might be fabricated, but it must not be in too large grain, nor too brittle: lath nails, tacks, and other small nails, bend when they are made of too soft iron; but, excepting this sole use, I do not see any need of making use of this brittle iron.

The best iron, i.e. that which is strongest, and confequently has not denfity, can bear one or two hundred ftrokes without breaking; and as it, nevertheless, is requifice to be broke for fervice, and as it would require much time, even with the affistance of steel shears, it is better to cut the hot bars about half their thickness with the hammer at the forge; this does not prevent the fmith to finish them, and spares much time to the flatter and workers of it. All the iron which I have broken with powerful strokes, heats fo much the more as it is stronger and oftener ftruck; it not only grows burning hot, but it takes a magnetical quality, as if it was rubbed on a loadstone. Being affured of the constancy of this effect, by feveral observations, I was defirous to see if I could produce it without percussion; for which purpose I took a fmall iron rod of my most pliable iron, and which I knew was very difficult to break, and having bent and rebent it, by the hands of a strong man feven or eight times fucceffively, I found the iron very hot at the point where it had been bent, and had all the virtue of a bar of iron touched with a loadstone. I

shall have occasion hereafter to return to this phanomena, which belongs partly to the theory of magnetism, and which I only mention here to demonstrate, that the more tenacious a matter is, the nearer it is of producing heat and all the other effects depending on it; and, at the same time, to prove, that the simple pressure producing the friction of the internal parts, is equivalent to the effect of the most violent percussion.

Iron is commonly foldered on itself, but the greatest caution is requisite, that it is not found a little weaker in the foldering part; for to unite and folder the two ends of a bar, they are heated to a white heat, when the iron is quite ready to melt, which does not happen without its lofing all its tenacity, and, confequently, its nerve: it cannot, therefore, regain it in the foldered part, but by the percussion of the hammers which two or three workmen cause to succeed as rapid as possible; but this percussion is very weak, and even flow in comparison of that of the hammer of the forge; so that the soldered part, however good the stuff may be, will have but little nerve, and often none at all, if the moment was not caught when the two pieces were equally hot, and if the motion of the hammer has not been quick and frong enough to unite them perfectly: so likewise, when important pieces are to be foldered, they should be done under the quickest strokes of the hammer. The folder in cannon is one of the most important things: Mr. Montbeillard, in the Memoir I have quoted, gives very good lights on this matter, and even decifive experiments. I am of his opinion, that as the maquette, or band, must be heated a number of times white to folder cannon its whole length, we must not use iron which is in its entire perfect state because it would waste by these frequent strong heats; but we must, on the contrary, chuse iron, which not being fo pure as it may be made, will rather gain the quality which the other would lofe by thefe heats;

but this article alone would require a great work made and directed by a man as enlightened as M. de Montbeillard, and the object is of fo great importance to the life of mankind, and the glory of the

state, that is merits the greatest attention.

Iron decomposes by humidity as by fire: it attracts the moisture of the air, is penetrated by its rust; that is to fay, converts itself into an earth without connection and without coherence; this conversion is made in a very little time in iron which is of a bad quality: that which is good, or whose surfaces are close or polished, defend themselves longer from ruft. but all are subject to this evil, which from the superficies foon reaches the internal parts, and in time destroys the whole body of the iron. It preserves itfelf much better in water than in air; and although we perceive its alteration by the black colour it takes there, it is not unnaturalized, but can be forged; whereas, that which has been exposed to the air for many years, and which the French smiths call fer lune, or moon iron, because they imagine it is devoured by the moon, can neither be forged nor be of any use; at least, unless it is revivified like the rust and faffron of steel, which commonly costs more than the iron is worth. This is, then, what the difference in the decompositions of iron consists: in that made by fire, the greatest part of the iron is burnt, and exhaled in vapours like other combustible matters; and there remains only a machefer or drofs which contains, like that of wood, a fmall quantity of matter very attractable by the loadstone, and which is real iron, but which appears of a nature fo fingular and fimilar, as I have observed the feruginous fand which is found in fuch great quantities in platina. The decomposition by humidity does not nearly diminish the mass of iron so much as combustion, but it alters all the parts thereof fo as to make them lofe their magnetical virtue, their coherence, and their metallic VOL. V. colour. colour. It is from this rust, or earth of iron, that the grain ore is in part composed: the water, after having attenuated these particles of rust, and having reduced them into molecules, carries them off, and deposes then by filtration in the bowels of the earth, where they re-unite in grain by a fort of chrystallisation, made by the mutual attraction of analogous molecules. As this rust also is deprived of its magnetic virtue, it is not surprizing, that the grain ore proceeding from it is equally deficient in it. This appears to demonstrate in a clear manner, that magnetism supposes the precedent action of the fire; that it is a particular quality that the fire gives to iron, and which the humidity of the air raises by decomposing it.

it is a particular quality that the fire gives to iron, and which the humidity of the air raises by decomposing it.

If a quantity of iron filings, which have not contracted any rust, be covered with water, and suffered to dry, they will be perceived to unite by that intermedium alone, so far as to form a mass so

very folid as not to be broken but by very powerful strokes: it is not, therefore, water precisely which decomposes iron, and produces ruft, but rather the falts and fulphureous vapours of the air; for iron is known to be eafily diffolved by acids and fulphur. By putting an hot iron rod to fulphur, the iron flows in a moment; and by receiving it in water, fmall shot are obtained which are no longer iron; for I have proved that they cannot unite by the fire, being a matter which can only be compared to pyrite, in which the iron appears to be equally decomposed by the fulphur, and this I think is the reason that almost on all the surface of the earth, and under the first body of its external strata, there are a fufficient quantity of these pyrites, whose grain resembles bad iron, but which contain only a very fmall quantity of it, mixed with much vitriolic acid.

and more or less fulphur.

FIFTH MEMOIR.

EXPERIMENTS on the Effects of obscure Heat.

To discover the effects of obscure heat, i. e. of heat deprived of light, slame, and open fire, as much as possible, I made some experiments, the results of which appear very interesting.

FIRST EXPERIMENT.

ABOUT the latter end of August, 1772, we began to put burning coals into the crucible of the great furnace used for melting iron ore to run it into fows; these coals dried the mortars which were made of clay, mixed with an equal portion of vitrescible sand. The furnace is twenty-three feet high. By the guelard (for fo the upper opening of the furnace is called) the live coals were thrown in which were brought from the small experimental furnaces: a fufficient quantity of coals were fucceffively put in to fill the bottom of the furnace as far as the cuve (for fo the greatest capacity of the furnace is called) which was feven feet two inches high from the bottom of the crucible: by this means we gave to the furnace a moderate heat, which was not felt in the upper part.

The 10th of September, all these coals reduced into cinders were taken out, and when the crucible had been thoroughly cleaned, some live coals were put therein, to 600 lb. weight; after this, sire was set to them, and the next morning, the surnace was continued to be filled to the amount of 4800 lb.

more, making in all 5400 lb. of coal.

During the time the entrance of the crucible was left open, and the tunnel well stopped to prevent the fire communicating to the bellows. The first impression

pression of this great heat was marked by two small cracks: the coal, nevertheless, although thoroughly lighted at bottom, was yet but low, and the furnace gave to the guelard but a very little smoke the same evening at six o'clock; for this upper opening was not stopped no more than the opening of the crucible.

At nine the same evening the slame reached the top of the surnace, and as it became very brisk in a short time, the opening of the crucible was closed at ten o'clock; the slame, although greatly abated by this suppression of air, was kept up the following day and night; so that next day, the 13th of September, towards four o'clock in the evening, the coals had sunk a little more than four feet. This void was filled immediately with 440lb. of coals; so the surface had been then charged with 5840lb. of coals.

Afterwards the upper opening was stopped with a broad and strong iron plate, closed round with a mortar made of clay and sand, mixed with powdered coals, and charged a foot thick with this powdered coal moistened. The surnace was lest thus stopped from the 13th to the 28th of the same month, during which time I remarked, that although there was no slame in the surnace, nor even any luminous fire, the heat still increased and communicated about the cavi-

ty of the furnace.

The 28th, at ten o'clock in the morning, we unflopped the upper opening with care, for fear of being suffocated by the sulphur of the coal: I remarked before opening it, that the heat had gained as far as four feet and an half in the thickness of the mass which formed the top of the surnace. This heat was not very great about the bure, (for so the upper part of the surnace is called, which rises above its platform); but in proportion as we approached the cavity, the stones were so very hot, that it was not possible to touch them a moment; the mortar in the joints of the

stones were in part burnt, and the heat was still much greater at the bottom of the furnace; for the stones below the tunnel were excessively hot through their whole thickness, to the height of sour or sive feet.

The moment the guelard of the furnace was unftopped, a fuffocating vapour iffued, from which it was requifite to get away, and which gave the head-ach to feveral of our affiftants. When this vapour was diffipated, we measured how much the coals had diminished in fifteen days thus deprived of air, and we found, that it had funk fourteen feet five inches; so that the furnace was amply as far as the cave.

Afterwards I observed the furface of this coal, and perceived a small flame which then commenced, being before absolutely black and without flame. In lets than an hour, this small bluish flame grew red in the center, and rose about two feet above the coals.

An hour after, having closed up the guelard, I opened the entrance of the crucible. The first thing which presented was not fire, as might have been presumed, but scoria proceeding from the coals, and which resembled light macheser, or dross: this macheser was in a pretty large quantity, and filled all the internal part of the crucible; and what appeared singular was, that although it was formed only by a great heat, it had intercepted this heat above the crucible, so that the parts of this macheser which were at bottom, may be said to have been only lukewarm; nevertheless, they adhered to the bottom and sides of the crucible, and had reduced some portion of it to three or four inches deep, into lime.

This machefer I had pulled out and put on one fide, as were the lime and other reduced parts. This calcination, made without flame, appeared to proceed partly from the action of these scoria: I thought that this fire without flame was too dry, and that if I had mixed a portion of vitrescible earth with the coals,

this earth would have ferved as food for the heat, and become melting matters which would have preferved

the furface of the furnace from calcination.

Be it as it may, we find by this experiment, that heat alone, i. e. obscure heat, inclosed and deprived of air as much as possible, produces, nevertheless, effects fimilar to those of the most active and luminous fire. We know that it must be very fierce to calcine stone. What I chose for the construction of the work and chimney of my furnace, was the least calcinable of all calcareous stones and the most resisting to fire: this stone likewise was cut and laid with care; the fmallest parts were a foot thick, and a foot and an half broad, by three and four feet long. In this large volume the stone is much more difficult to calcine, than when it is reduced into fmall pieces: yet, this heat not only calcined these stones near half a foot deep in the narrowest and coldest part of the furnace, but also burnt at the same time the mortar made of clay and fand without melting them, which last would have pleased me better, because then the joints would have been preferved whole; whereas, the heat having followed these joints, still calcined the stones on them. But to render the effects of this obfcure and concentrated heat better understood; first, that the furnace being twenty-eight feet thick of two facies, and twenty-four feet thick of the two other facies; and the cavity where the coal was contained being only fix feet in its largest breadth: the walls were nine feet thick of mason's work of lime and fand; that, confequently, no air could be supposed to pass through. Secondly, this cavity with coals, having been stopped at bottom, with clay mixed with fand a foot thick, and to the tunnel with the like mortar. It is not to be prefumed, that any air could enter by these two openings. Thirdly, that the gue lard of the furnace being that up with a strong iron plate, heated and covered with the fame mortar about

fix inches thick, and befides furrounded and covered with afhes mixed with this mortar fix inches high, all access of air was stopped through this opening: it may, therefore, be afferted, that there was no air circulating in all this cavity, the capacity of which was 330 cube feet, and that having filled it with 5400 lb. of coals, the fire in this cavity could only be fed with the small quantity of air contained in the intervals which were left between the coals. As this matter thrown one on the other left great voids, supposing half, or even three-fourths, there was then in this cavity only 165, or at most 248 cubical feet of air. the fire of the furnace, excited by the bellows, confumed this air in less than half a minute, and yet it feems to have been fufficient to support the heat for fifteen hours, and increase it nearly as much as the open fire, fince it calcined stones four inches at bottom, and more than two feet in the middle, and throughout the whole extent of the furnace. As this appeared to me inconceivable, I immediately added to the 248 cube feet of air, all the vapour of the humidity of the walls, which the concentrated heat did not fail to attract, and of which it was scarcely possible to make a just estimation. These are the only aliments, (air and vapours,) which this very great heat confumed in fifteen days; for little or no air difengaged from the coals in burning, although more than one third of the weight of oak well dried is loft in combuftion.* This fixed air contained in wood is driven off by the first operation of fire, which converts it into charcoal; and if any remains, it is only in fo fmall a quantity, that it cannot be regarded as the fupplement of air which remained for the fupport of the fire: thus, this very great heat, and which was increased to the point of deeply calcining the stones, was supported only by 248 cubical feet of air, and by the moift vapours of the walls: and if we should suppole

^{*} Dr. Hale's Vegetable Statics, page 152.

pose the successive product of this humidity to be one hundred times more considerable than the volume of air contained in the surnace, that will make only 24800 cubical feet of vapours; a quantity an open fire, animated by the bellows, would consume in less than thirty minutes, whereas this heat consumes it

only in fifteen days.

What is still necessary to observe is, that this open and animated fire would have confumed in eleven or twelve hours the 3600 lb. of coal, which the obscure heat confumed only in fifteen days; it, therefore, had only a thirtieth part of the food of the open fire, fince there was thirty times as much time employed for the confumption of the combustible matter; and, at the same time, about 720 times less air or vapours: nevertheless, the effects of this obscure heat were the fame as those of the open fire; for it would have required fifteen days for this fierce and animated fire to calcine stones to the same degree as they were to heat alone, which demonstrates, on one hand, the immense deperdition of the heat when it exhales with the vapours and flame; and, on the other, the great effects that may be expected from its concentration, coercion, or detention; for this retained and concentrated heat having produced the fame effects as an open and fierce fire, with thirty times less combustible matter, and 720 times less air, and being supposed in a compound ratio of these two elements, it must be concluded, that in our great furnaces 27000 times more heat will be loft than will be applied either to the ore or fides of the furnace; fo that it would be imagined. that the reverberatory furnaces, where the heat is more concentrated, ought to produce the fiercest fire. Nevertheless, I have proved the contrary, our iron ore not being even agglutinated by the reverberatory fire of the glass-house in Rouelles, in Burgundy, whereas it melts in twelve hours in my wind furnace: this difference partakes of the principle which I have given.

given. Fire, by its velocity or volume, produces quite different effects on certain substances, such as iron ore; whereas, on others, such as calcareous stone, it may produce the like. Fusion, in general, is a ready operation, which must have more relation with the velocity of fire than calcination, which is always slower, and which must in many cases have more affinity with the volume of fire, than to its velocity. We shall find by the following experiment, that this retained and concentrated heat had no effect on the iron ore.

SECOND EXPERIMENT.

IN the same furnace, I melted iron by keeping it continually filled with coals, but without any ore, in order to extract all the melted matter; and when I was affured no more remained, I shut close the openings, and covered them with brick and mortar made of clay and fand: I afterwards had as much ore put upon the coal as could enter into the void space on the top of the furface. There entered this first time 1620lb. after which I closed it up as before, with the addition of powdered coal. It may be well unagined what an immense heat I inclosed thus in the furnace, all the coal being lighted before I stopped the blall of wind; all the stones were red; and this heat could only exhale by two finall cracks in the furnace, which I stopped with mortar: three days after, I unstopped the guelard, and saw with furprize, that the burning coals, although compressed, had only funk fixteen inches in three days; I immediately replenished these fixteen inches with 1500 lb. of ore. Three days afterwards, I opened this fame guelard, and found the same space of fixteen inches, which I again replenished with 1500lb. of ore: thus, there was 4620 lb. on the coals. Six days after, I opened the guelard for the third time, and VOL. V. found, found, that during these six days, the coals had only sunk twenty inches, which was replenished with 1600lb, of ore. At last, nine days after it was opened for the fourth time, I saw, that during these nine days the coals had only sunk nine inches. The guelard was again shut with the same precautions, and the next morning I took the brick-work which covered the opening at bottom, keeping always the guelard close shut, to avoid the current of air which would have instanced the coals. The first thing that was drawn out were pieces reduced to lime in the surnace work; some small pieces of macheser were also found, and some others of a bad digested susion, and about 1½lb. of very good iron, formed by congulation.

After having drawn all these matters out, the coals were suffered to fall: the first that appeared was scarcely red, but as soon as it selt the air, it became very red: it was immediately drawn out and extinguished with water. The guelard being still kept shut, all the coals were drawn out by this opening, and also all the ore which I had loaded it with. The quantity of coals drawn from the surnace amounted to 1151 bushels; so that in these twenty-two days violent heat, only seventeen bushels were consumed; for the surnace contained only 135, and as there were fixteen inches and an half void space when it was stopped, we must deduct two bushels which would have been necessary to fill this void.

Aftonished at this small consumption of coal in this time, I inspected them more narrowly, and found, that although they had lost little of their volume, they had lost much of their mass; and that although the water with which they had been extinguished, added to their weight, they were still a third lighter than when they were thrown into the furnace; notwithstanding which, having sent them

to the iron-worker's, they were good enough to heat the small bars of iron to a whiteness. estribbom vyb

The ore was taken out at the same time as the coals, and carefully separated therefrom: the very violent heat which it had endured fo long, had neither melted, burnt, nor even agglutinated it; it was only become more gloffy. The vitrescible fand, and the small flints mixed with it, were not melted, and it appeared not to have loft only the humidity it contained before, for it had hardly lost a fifteenth of its weight, and about a twentieth in volume, which

last was lost in the coals.

From this experiment refults, first, That the most violent heat concentrated for a long time, cannot, without the aid and renewal of air, melt iron ore, nor even vitrescible fand, whereas a much less heat of the fame kind can calcine calcareous matters. Secondly, That coals penetrated by heat or fire, diminishes in mass a long time before it diminishes in volume, and that its most combustible parts are the first which it loses; for, by comparing this second experiment with the first, from whence does it proceed, that the fame quantity of coals confume quicker by a moderate heat, than by a fiercer, when both are alike deprived of air, alike retained and concentrated in the same close vessel? In the first experiment, the coals, in a cold cavity, which had proved only the flight impression of a fire stifled the moment the flame was perceived, had, nevertheless, diminished two thirds in fifteen days; whereas, the same coals, inflamed as much as possible by the blast of the bellows, and receiving the immense heat of the red stones around it, did not diminish a sixteenth in twenty-two days. This would be inexplicable if it was not confidered, that in the first case the coals had all their denfity, and all the combustible parts; whereas, in the second, when it was in the strongest incandescence, all its most combustible parts were burnt.

burnt. In the first experiment, the heat, at first very moderate, increased in proportion with the combuftion, and communicated more and more to the whole mass of coals. In the second experiment, the heat diminished in proportion as the coals left off burning, and could no longer afford fo much hear, because its combustion was very forward at the time it was thut: this, then, is the true cause of the different effects. The coal in the first experiment containing all its combustible parts, burns better, and confumes faster than that of the second experiment, which fearcely contained any more combustible matter, and could not increase its fire, nor even support it at the same degree, but by borrowing from the furnace walls. It is for this reason alone, that combustion diminishes; and that, in the whole, it had been much less and flower than the other which increafed and was made in less time. When all access of air is obstructed, and the inclosed matters contain only little or nothing in their fubflance, they will not confume, however yielent the heat may be; but if there remains a certain quantity of air between the interffices of the combustible matter, it will confume for much the quicker, and for much the more it will be able to furnith itself with a greater quantity of air. Thirdly, There refults still from these experiments, that the most violent heat, as soon as it is no longer fed, produces less effect than the smallest heat which is supplied: the first may be faid to be a dving heat, which only causes itself to be perceived by its deperdition; the other is a living heat, which increases in proportion to the food it confumes. To discover what this dying heat, i. e. this heat deprived of all food might produce, I have made the following experiment.

THIRD EXPERIMENT.

AFTER having drawn from the furnace all the coals contained therein, and entirely emptied it of ore, and of every other matter, I carefully inclosed it as before, all the stones of the fides being yet hot: the air could not, therefore, enter into the furnace to cool it, and the heat could not pals out but through walls nine feet thick. Observing, therefore, what would refult therefrom, I perceived, that the effect of the heat was carried upwards; and that, although this heat was not living fire, or fed by any combustible matter, it foon reddened the strong iron plate which covered the guelard: that this incandescence given by the obscure heat to this large piece of iron, communicated by contact to the whole mass of coal dust, which covered the mortar of this plate, and fet fire to fome wood which was put thereon. Thus, the fole evaporation of this heat, produced the fame effect as live heat well supplied: this heat always tending upwards, and uniting at the opening of the guelard below the iron plate, made it red, luminous, and capable of inflaming combustible matters; from whence it must be concluded, that by increasing the mass of obscure heat, light may be produced in the fame manner, as by augmenting the mass of light we produce heat: from hence these two fubstances are reciprocally convertible one into the other, and both necessary to the element of fire.

When this iron plate which covered the upper opening of the furnace, and which the fire had reddened, was taken off, a flight vapour iffued out, which appeared inflamed and diffipated in a moment: I then observed the stones of the furnace calcined very deeply; and, in fact, having suffered the furnace to cool, they were calcined to the depth of two feet and an half, which could only proceed from

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the heat which I had shut up in my first experiments.

From this experiment we may learn the method of burning stone, and making of lime at a less expence, i.e. to diminish greatly the quantity of wood by making use of a well-inclosed furnace instead of open ones: a small quantity of coal will be only requisite in a few days to convert into lime, all the stones in the surnace, and even the walls of the surnace, to more than a foot thick, if it was very nicely inclosed.

As foon as the furnace was cool enough to fuffer the workmen to work, all the internal part was obliged to be demolished. From a circular thicknels of four feet, we extracted fifty-four bushels of lime, on which I made the following experiments: 1. This stone was not so light as stone calcined in the common road, which loses half its weight, whereas mine lost only three-eighths. 2. It did not so greedily drink up the water as the other, nor gave at first any fign of ebullition; but in a short time it fwelled and divided, so that there was no occasion to stir it like the common. 3. Its taste was sharper, and it confequently contained more fixed alkali. 4. It is infinitely better, more binding, and ftronger. 5. It did not extinguish in the air till after some time, whereas only a day or two is required to reduce common quick lime into powder in the open air; but this refisted the air for five or fix weeks. 6. Instead of being reduced into a dry powder it preserved its volume, and when divided by breaking, it appeared ductile and penetrated by a fat and binding moisture, which can only proceed from the moisture of the air which the stone powerfully attracted and absorbed during the five weeks. On the whole, the lime commonly taken from forge furnaces has the same properties; therefore, an obscure and

and gentle heat produces the same effects as the fiercest fire.

From this demolition of the internal part of the furnace, we had 232 quarters of stone, all more or less deeply calcined: these quarters were commonly four feet in length, and most of them reduced into lime as far as eight inches, and others to two feet, and even two feet and an half; and this calcined portion separated easily from the rest of the stone which was found, even harder than when first laid. This observation induced me to make the following experiment.

FOURTH EXPERIMENT.

I caused three pieces of this stone to be weighed. and I compared the specific weight of them with three other pieces of nearly the fame volume, which was taken from the same stone which had not been used for the construction of the furnace, nor consequently heated, but which had been cut from the same quarry nine months before. I found, that the specific weight of the heated stones was constantly greater than that of the fame stone not heated by an 81 on the first piece, 90 on the second, and 85 on the third; therefore, the stone heated almost to calcination gained at least an 86th of the mass, whereas it loses 3-8ths by calcination, which supposes only a farther degree of heat. This difference can only proceed, that from a certain degree of violent heat, or fire, all the air and water transformed into fixed matter in the stone, to retake their first nature, elasticity, and volatility; and that from hence they difengage themselves from the stone, and fly off in vapours: a fresh proof, that the calcareous stone is in a very great part composed of fixed air and water, caught and transformed into a folid matter by the animal filter.

After these experiments, I made others on this same stone heated to a less degree: for this purpose I took off three pieces from the external parts of the tunnel, in a part where the heat was nearly ninety-sive degrees, because the sulphur applied against the wall melts there, and that this degree of heat is nearly to that which suses sulphur. I sound by three similar trials, that this stone, thus heated for sive months, had increased in specific weight a 69th, i.e. almost a quarter more than that which had endured the next degree of heat to calcination; and I conclude from this, that the effect of the calcination is preparing in the stone which has undergone the greatest fire; whereas, that which had only proved a less heat, had retained all the fixed parts it had

deposited there.

To fatisfy myself fully on this subject, and discover whether all calcareous stones increased in specific weight by a heat constantly and a long time applied, I made fix new trials on two other kinds of stone; that with which the infide of my furnace was confructed, and which ferved for the preceding experiments, called in the country Fire-stone, because it refifts the action of the fire more than all other calcareous stones: its substance is composed of small calcareous gravel, bound together by a ftrong cement which is not very hard, and which leaves some void interstices; its weight is, notwithstanding, a 20th greater than other calcareous stones. Having tried feveral pieces in the fire of my stoves, double the time was required to calcine them, than to reduce other stones into lime. It may, therefore, be afferted, that the experiments were made on calcareous stone which had the greatest refistance to fire. The stones to which I compared it, were also very good calcareous ftones, fome almost of as fine grains as marble; the other coarfer, but both compact and full: full: both form excellent grey lime, stronger and more binding than common lime which is white.

By weighing three heated pieces, and three not heated, of the stone whose grain was finest in water and air, I found it had gained a 56th in specific weight, by sive months application of a heat of ninety degrees; and by comparing them with other pieces of stone exposed to the open air, I found that one had increased a 60th, the second a 62d, and the third a 65th: thus, this very fine-grained stone had augmented in specific weight nearly one third more than the stone heated to the next degree to calcination, and also to about one seventh more than the stone heated to ninety-sive degrees.

The fecond stone, whose grain was not so fine, formed an entire course of the external vault of the furnace; and I chose the pieces from a part where they had undergone the same degree of 95 for five months: I found that one of the three pieces had increased a 54th, the second a 63d, and the third a 66th, which gives but the medium propor-

tion, a 61 increase in specific weight.

The result of these experiments are, first, That all calcareous stone heated for along time, acquires mass, and becomes heavier: this augmentation can only proceed from the particles of heat which penetrate and unite with it by their long stay, and which from thence becomes a constituent part under a fixed form. Secondly, That this augmentation of specific weight being a 60th, or 56th, or 56th, only varies by the nature of the different stones: that those whose grain are the finest, are those whose mass is increased the most by heat, and in which the pores being smaller, fixes more readily, and in a greater quantity. Thirdly, That the quantity of heat which fixes in the stone is yet much greater than the augmentation · of the mass denotes: for the heat before it is fixed in the stone, drove out all the moist parts it con-. Vol. V. tained. tained. It is well known, that by distilling calcareous stone, pure water is drawn from it to the amount of a 16th of its weight; but as a heat of ninety-five degrees, although applied for five months, might, nevertheless, produce in this respect less effects than a violent fire applied to the vessel in which stone is distilled, reducing this quantity of water to ½ or even by the heat of 95 degrees, we cannot difallow, but that the quantity of heat fixed in this stone, is not at first indicated by the augmentation of the specific weight, and to a 54th for the quarter of the quantity of water it contained, and which this heat fends out; fo that it may be afferted, that the heat which penetrates stone, fixes therein in a sufficient quantity to increase the mass at least a 13th, even in the suppofition, that it did not drive off all this time only 4 of the water contained in the stone.

FIFTH EXPERIMENT.

ALL calcareous stones, whose specific weight increases by the long application of fire, acquires by this kind of drying more hardness than they before had. Being defirous of discovering whether this hardness would be durable, and if they would not with time lose not only this quality, but the increase of denfity they had acquired by heat, I caused to be exposed to the air many parts of the three kinds of flones which had been used for the preceding experiments, and which had been all more or less heated for five months. At the end of fifteen days, during which time it rained, I had them struck with a hammer, and the workman, as well as me, perceived that the fire-stone, which was the most porous, and whose grain was the coarfest, was not so hard, and suffered itself to be easier worked. But the two other kinds, and especially that whose grain was finest, had retained the same hardness; nevertheless, they lost it in in fix weeks. Having tried them hydrostatically, I observed, that they had also lost a pretty great quantity of the fixed matter which the heat had deposited there: nevertheless, at the end of several months they were always specifically heavier by a 150th or a 160th part, than those which had not been heated. The difference, then, growing too dissicult to be observed, I was obliged to stop here; but, I am persuaded, that by much time these stones would have lost all their acquired weight: it is the same with the hardness, after being exposed some months to the air, they were worked as easily as other stones of the same kind which had not been heated.

This experiment evinces, that the particles of heat which fix in stone, are only united there by force: that it retains them a long time after it is entirely cold, if preferved from all moisture: it loses' them nevertheless, by degrees by the impressions of the air and rain, without doubt because air and water have more affinity with stone than the parts of heat lodged there: this fixed heat is no longer active; it may be faid to be dead and entirely passive; hence, far from driving off the humidity that deftroys the heat in its turn, and takes possession of the places it has ceded to it: but in other matters, which have not fo much affinity with water as calcareous stone. this heat, once fixed, does it not remain there conantly and reciprocally? This is what I have endeavoured to be afcertained of by the following experiment.

SIXTH EXPERIMENT.

I took feveral pieces of cast iron, which I had broken in the middle, which had served several times to support the chimney work of my surnace. The pieces of this cast iron, when broke, did not separate

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from the rest of the matter but by reiterated strokes; whereas, the iron of this same cast, but which had not undergone the action of the fire, were very brittle, and broke in pieces: I then perceived, that this heated cast had acquired much more hardness and tenacity than before, much more even in proportion than the calcareous stones had acquired. By this first mass, I judged that I should find a still greater difference in the specific weight of this cast: and, in fact, the first piece I tried in the hydrostatical balance, weighed 4lb. 4 ounces 3 drams in the air: the fame piece weighed 3lb. II ounces 21 drams in water, i.e. the difference is 721 drams. The water I used weighed exactly 70lb. to the cubical foot, and the water displaced by the cast iron, 721 drams: therefore, 721 drams of water, displaced by the cast iron, are to 70lb. cubical foot of water, as 547 drams of the piece of cast iron, are to 5281b. 2 ounces I dram 47 grains weight of the cube foot of this cast iron: this weight also greatly exceeds that of this fame iron when it has not been heated: it is a white cast iron which is brittle, and whose weight is only 495 or 500lb. Thus, the specific gravity is found augmented from 28 to 500, by this very long application of heat, which makes about an 18th of the mass. I am certain of this great difference by five successive trials, for which I always took care to pick out pieces which weighed 41b. each at least, and compared them one by one with pieces of the fame figure, and nearly of the fame volume; for although the difference of fize should appear here to have no influence on the refult of the hydrostatic balance, nevertheless, those who are used to heat it, will perceive, as well as me, that the refults are always juster when the fizes of the matters compared are not much greater than each other. Water, however fluid it appears, nevertheless, has a certain small degree of tenacity which has more

more or less influence on the greater or less fizes: besides, there are very sew matters which are perfectly homogenous, or alike in weight, in all the external parts of the matter submitted to the trial; therefore, to obtain a result on which we may precisely rely, we must always compare pieces of nearly the same volume and figure: for if, on the one hand, a globe of iron of 2lb. is weighed; and, on the other, an iron plate of the same weight, we shall find their specific weight to be different in the hydrostatic balance, although it was really the same.

I think, that whosoever will reflect on the preceding experiments and results, will not disallow that the heat applied a very long time to the different bodies it penetrates, does not deposit in their inside a very great quantity of particles which become constituent parts of their mass, and which unite and adhere thereto so much the more as matters are found to have more affinity and other connections of nature with them. Finding myself also furnished with these experiments, I did not fear advancing in my Treatise of the Elements, that the molecules of heat fixed themselves in all bodies, as those of light and air do, when it is accompanied with heat or fire.

SIXTH MEMOIR.

EXPERIMENTS on Light, and on the Heat it can produce.

ARTICLE I.

Invention of Mirrors to burn at great Distances.

HE story of the burning-glasses of Archimedes is famous: he invented them for the defence of his country; and, according to the ancients, he reflected

flected the fire of the fun on the enemy's fleet, which reduced it into ashes as soon as it approached the ramparts of Syracusus: but this story, of which no doubt has been made for fifteen or fixteen centuries. has been contradicted and treated as fabulous in these latter ages. Descartes, born to judge and surpass Archimedes, has in a mafterly manner spoken against him: he has denied the possibility of the invention. and his opinion has prevailed over the testimonies and credit of the ancients. Modern phylicians, either through a respect for their philosopher, or through complaifance for their cotemporaries, have given it to the fame opinion. Nothing is allowed to the ancients but what cannot be avoided. Determined, perhaps, by these motives, of which selflove too often is the abettor without our knowledge of it, have we not naturally too much inclination to refuse what is due to our predecessors; and if, in our time, more is refused than any other, is it not that-by being more enlightened, we think we have more right to fame, and more pretentions to superiority?

Be it as it may, this invention was the cause of many other discoveries of antiquity at present vanished, because the facility of denying them has been preserved to the trouble of sinding them out; and the burning-glasses of Archimedes were so decried, that it does not appear possible to re-establish the reputation: for, to call the judgment of Descartes in question, something more is required than evasions, and there only remains one sure decisive mode, but, at the same time, dissibute and bold, which was to undertake to discover glasses which might produce the like effects. I had for a long time conceived an idea of it, and I will voluntarily acknowledge the greatest dissibute to find it possible, since I have succeeded in the execution be-

yond even my expectations.

I have, therefore, fought after the mode of making mirrors to burn at a great distance, as from 100 to 300 feet. I knew, in general, that reflecting mirrors, never have burnt farther than 15 or 20 feet, and with refringent, the distance was still shorter; and, I perceived, it was impossible by practice to form a metal or glass mirror with such exactness as to burn at these great distances. To burn, for example, the sphere must be 800 feet diameter; therefore, we could hope for nothing of that kind in the common mode of working glasses, and I soon persuaded myfelf, that if we could find a new method to give to great pieces of glass, or metal, a curve sufficiently slight, there would still result but a very inconsiderable advantage, as I shall hereafter mention.

But, to proceed regularly, I shall first see how much light the sun loses by reflection at different distances, and what are the matters which reslect it the strongest: I first found, that glasses when they are polished with care, reslect the light more powerfully than the best polished metals, and even better than the compounded metal with which telescope mirrors are made; and that although there are two reslections in the glasses, they yet give a brighter and clearer light than metal, which produces a co-

loured light.

Secondly, By receiving the light of the sun in a dark place, and by comparing it with this light of the sun reflected by a glass, I found, that at small distances, as sour or sive seet, it only lost about half by reslection, which I judged by letting a second reslected light fall on the first; for the briskness of these two reslected lights appeared to me to be equal to that of direct light.

Thirdly, Having received at the distances of 100, 200, and 300 feet, this light reflected by great glasses, I perceived, that it lost nothing of its strength

strength by the thickness of the air it had to pass

through.

I afterwards tried the same experiments on the light of candles, and to assure myself more exactly of the quantity of weakness that reslection causes to

this light, I made the following experiments:

I feated myself opposite a glass mirror with a book in my hand, in a room where the darkness of the night could not permit me to distinguish a fingle object. In an adjoining room, I lit a candle at about 40 feet distance, and I approached it nearer and nearer, till I could read the book: the distance was then 24 feet. Afterwards, having turned the book, I endeavoured to read by this reflected light, and by a parchment intercepted the part of the light which did not fall on the mirror, in order to have only the reflected light on my book. I was obliged to approach the candle nearer, which I did by degrees, till I could read the same characters clearly by the fame light, and then the distance from the candle, comprehending that of the book to the mirror, which was only half a foot, I found to be in all 15 feet. I repeated this feveral times, and had always nearly the fame refults; from whence I concluded, that the strength or quantity of direct light is to that of reflected light, as 576 to 525; therefore, the light of five candles received by a flat glass, is nearly equal to that of the direct light of two.

The light of a candle, therefore, loses more by reflection than the light of the sun; and this difference proceeds from the rays of the sun which come from the candle as from a center, falls more obliquely on the mirror, than the rays of the sun which come almost parallel. This experiment, therefore, consirms what I had at first found, and I hold it certain, that the light of the sun loses only half

by its reflection on a glass mirror.

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This first knowledge, of which I had need, being acquired, I afterwards fought what became of the images of the fun when received at great distances. To understand well what I am going to fay, we must not, as is generally done, consider the rays of the fun as parallel; and it must be remembered. that the body of the fun occupies an extent of about thirty-two minutes: that, confequently, the rays which iffue from the upper edge of the disk, falling on a point of a reflecting furface; the rays which issue from the lower edge, falling also on the same point of this furface, they form between them an angle of thirty-two minutes in the incidence, and afterwards in the reflection; and that, consequently, the image must increase in size, in proportion as it is farther distant. Attention must likewise be paid to the figure of these images: for example, a plain fquare glass of half a foot, exposed to the rays of the fun, will form a fquare image of fix inches, when this image is received at the distance of a few feet: by removing farther and farther off, the image is feen to increase, afterwards to become deformed, then round; in which state it remains still increasing in fize, in proportion as we are more distant from the This image is composed of as many of the fun's disks as there are physical points in the reflect. ing furface: the middle point forms an image of the disk, the adjoining points form the like, and of the same fize which exceed a little the middle disk: it is the same with the other points, and the image is composed of an infinity of disks, which furmounting regularly, and anticipating circularly one over the other, form the reflected image, of which the middle point of the glass is the center.

If the image composed of all these disks is received at a small distance, then their extent being somewhat larger than that of the glass: this image is of the same sigure, and nearly of the same extent as the

Vol. V. Z glass:

glass: but when the image is received at a great distance from the glass, where the extent of the disks is much greater than that of the glass, the image no longer retains the same figure as the glass, but becomes necessarily circular. To find the point of distance where the image loses its square figure, we have only to feek for the distance where the glass appears under an angle, equal to that the fun forms to our fight, i.e. an angle of 32 minutes, and this distance will be that where the image will lose its fquare figure and become round: for the disks, having always an equal line to the femi-circle, which measures an angle of 32 minutes for a diameter, we shall find by this rule, that a square glass of fix inches, loses its square figure at the distance of about 60 feet; and that a glass of a foot square, loses only 120 feet, and so on of the rest.

By reflecting a little on this theory, we shall no longer be associated to find, that at very great distances, a large and small glass affords nearly an image of the same size, and which only differs by the intensity of the light, we shall no longer be surprized that a round, square, long, or triangular glass, or any other sigure always yields round images; and we shall see evidently, that images do not increase and lessen by the dispersion of light, or by the loss in passing through the air, as some physicians have imagined; but that, on the contrary, it is only occasioned by the augmentation of the disks which always occupy a space of 32 minutes to whatever

distance they are removed.

So likewise we shall be convinced, by the exposition of this theory, that curves, of whatsoever kind they be, cannot be used with advantage to burn at a

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^{*} This is the reason that the small images which pass betwixt the leaves of high and full trees, and which falling on the walk, are all oval or round.

great distance, because the diameter of the socus can never be smaller than the chord which measures an angle of 32 minutes; and that, consequently, the most perfect concave mirror, whose diameter is equal to this chord, will never produce double the effect of this plain mirror of the same surface; and if the diameter of this curved mirror was less than this cord, it would scarcely have more effect than a

plain mirror of the fame furface.

When I had well comprehended the above, I foon perfuaded myfelf no longer to doubt, that Archimedes could not burn at a distance but with plain mirrors: for, independently of the impossibility they were then in, and wherein we are at present, of making concave mirrors with fo large a focus, I perceived, that the reflection I have just made, could not have escaped this great mathematician. Besides, I thought, that, according to every appearance, the ancients did not know how to make large maffes of glass; that they were ignorant of the art of burning it to make large glaffes; that they had only the method of blowing it, and making bottles and vafes; and I readily perfuaded myself, that it was with plain mirrors of polished metal, and by the reflections of the sun, that Archimedes had burnt at a But as I perceived, that glass mirrors reflected the light more powerfully than the most polished mirrors, I thought to construct a machine to coincide in the same point the reflected images by a great number of these plain glaffes; being well convinced, that this was the fole mode of fucceeding.

Nevertheless, I had still some doubts remaining, which appeared to me well founded. Let us suppose, that the burning distance was 240 feet, I perceive clearly that the focus of my mirror cannot have a less than two feet diameter. Then, what will be the extent I shall be obliged to give to my affemblage of plain mirrors, to produce a fire in so great a

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focus? It might be so great, that the thing had been impracticable in the execution; for, by comparing the diameter of the focus to the diameter of the mirror, in the best reslecting mirrors, I observed, that the diameter of the Academy's mirror, which is three feet, was 108 times bigger than its focus, which was no more than four lines, and I concluded, that to burn as strong at 240 feet, it was necessary that my assemblage of mirrors had 216 feet diameter, since the focus should be two feet; now, a mirror of 216 feet diameter was certainly an impossible thing.

In fact, this mirror of three feet diameter burnt strong enough to melt gold, and I was desirous to fee how much I had gained by reducing its action to the burning of wood only: for this purpose I used circular zones of paper on the mirrors to diminish the diameter, and I found, that there was no longer power enough to inslame dry wood when its diameter was reduced to little more than four inches; therefore, taking five inches, or fixty lines, for the diameter necessary to burn with a focus of four lines, I cannot help concluding, that to burn equally at 240 feet, where the focus should necessarily have two feet diameter, I should require a mirror of thirty feet diameter, which appeared still as impossible, or at least impracticable.

To fuch positive reasons, and which others would have regarded as demonstrations of the impossibility of the mirror, I had only a supposition to oppose, an old supposition, on which the more I had reflected, the more I was persuaded, that it was not without soundation; which is, that the effects of heat might possibly not be proportional to the quantity of light; or, what amounts to the same, that at the equal intensity of light, large socus's must burn

brifker than the small.

By estimating heat mathematically, it is not to be doubted, but that the power of focus's of the same

length is proportional to the surface of the mirrors. A mirror, whose surface is double that of another, must have the same fized focus, and this focus must contain double the quantity of light which the first contained; and in the supposition, that effects are always proportional to their causes, it had always been thought that the heat of this second focus should be double that of the first.

So likewise, and by the same mathematical estimation, it has always been thought, that at an equal intensity of light, a small focus ought to burn as much as a large one, and that the essect of the heat ought to be proportional to this intensity of light: insomuch, (says Descartes) that glasses, or extremely small mirrors, may be made, which will burn with as much violence as the large. I at first thought, as I observe, that this conclusion, drawn from mathematical theory, might be found salse in the practice, because heat being a physical quality, of the action and propagation of which we know not the laws: it seemed to me, that there was some kind of temerity to estimate thus essects by a simple speculation.

I had, therefore, once more resource to experiments. I took metal mirrors of different socus's and different degrees of polish, and by comparing the different actions on the same sufficient combustible matters, I found, that at an equal intensity of light, large socus's constantly have more effect than small, and often produce only a moderate heat: I disco-

vered the same with refracting mirrors.

The reason of this difference is easy to be given, if we consider, that heat communicates nearer and nearer, and disperses, if I may say so, when it is even applied on the same point: for example, if we let the socus of a burning-glass fall on the center of a crown piece, and that this socus was only a 22d of an inch diameter, the heat produced on the center of the crown disperses and extends over and through-

out the whole piece: thus all the heat, although used at first to the center of the crown, does not stop there, nor can produce so great an effect, as if it remained there wholly. But if, instead of a socus of a 12th of an inch, we let fall a socus of equal intensity on the whole crown, every part being alike heated, in this latter case, there is not only the loss of heat, but even its grain and augmentation; for the middle profiting of the heat with the other points which surround it, the crown will be melted in this latter case, whereas, in the first, it will be only

flightly heated.

After these experiments and reslections, I selt the hope of my success to make mirrors to burn at a great distance prodigiously to increase; for I began no longer to dread, as before, the great extent of the socus; I was persuaded, on the contrary, that a socus of a considerable breadth, as those of two feet, and in which the intensity of the light would not be near as great as in a small focus of four lines, or 4-12ths of an inch, might, nevertheless, produce inflammation, and with more power; and that, consequently, this mirror, which, by mathematical theory, ought to have at least thirty feet diameter, would be reduced to a mirror of eight or ten feet at most, which is not only a possible but even a very practicable thing.

I then thought feriously to execute my project: I had at first a design of burning at 2 or 300 feet distance with circular or hexagon glasses, of a square foot in surface, and I was desirous of having four iron carriages for them, with screws to each to move them, and a spring to adjust them; but the too considerable expence this adjustment exacted, made me quit this idea, and I took two common glasses of six inches by eight, and a wooden adjustment, which, in fact, is less solid and precise, but the expence is more agreeable. M. Passement,

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whose abilities in mechanism is known to the Aca-

demy, took upon himself this performance.

It is sufficient to say, that it was at first composed of 168 glasses of six inches by eight each, about 4-12ths of an inch distant from each other; these glasses moved in all directions, and the four lines of space between them not only served for the freedom of this motion, but also to let the operator see the place where he was to conduct his images. By means of this construction, 168 images could be thrown on one point, and, consequently, burn at several distances, as at 20, 30, and to 150 feet, by increasing the size of the mirror, or by making other mirrors like to the sirst, we are certain of throwing fire to still greater distances, or to increase as much as we please the force or activity of those first distances.

It is only to be observed, that the motion here spoken of, is not very easy to be executed, and that also there is a very great choice to be made in the glasses; for they are not all equally good, though they appear so at the first inspection. I have been obliged to pick out of the 500 to have the 168 I made use of. The method of trying them is to receive at 150 feet distance the reslected image of the sun, as a vertical plane; we must select those which give a round and terminated image, and put aside the rest, which are very numerous, and whose thicknesses being unequal in different parts, or the surface a little concave or convex, have images badly terminated, double, treble, oblong, &c. according to the different desects found in the glasses.

By the first experiment I made the 23d of March, 1747, at noon, I set fire to a plank of fir at fixty-fix seet distance, with forty glasses only, i.e. with about a quarter of the mirror: but it must be observed, that not being yet mounted, it was very disadvantageously placed, forming an angle with the

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fun of twenty degrees declination, and another of

more than ten degrees inclination.

The same day I set fire to a pitchy and sulphureous plank at 126 feet distance, with eighty-eight glasses, the mirror being still placed disadvantageously. It is well known, that to burn with more advantage, the mirror should be directly opposed to the sun, as well as the matters to be inflamed; so that, by supposing a perpendicular plane on the plane of the mirror, it must pass by the sun, and, at the same time, through the midst of combustible matters.

The 3d of April, at four o'clock in the evening, the mirror being mounted, produced a flight inflammation on a plank covered with pitch at 138 feet distance, although the sun was weak and the light pale. It is requisite to be careful, when we approach the spot where the combustible matters are, and not look on the mirror; for if, unfortunately, the eyes should meet the focus, inevitable blindness will enfue.

The 4th of April, at eleven in the morning, the fun being watery, and the sky cloudy, yet it produced with 154 glasses so considerable an heat at 158 feet, that in less than two minutes it made a deal plank to smoke, and which would certainly have flamed, if the sun had not suddenly disappeared.

The ensuing day, the 5th of April, at three o'clock in the afternoon, we set fire at 150 feet distance, and in a minute and an half to a plank sulphured and mixed with coals, with 154 glasses: when the sun is powerful, only a few seconds is required to produce inflammation.

The 10th of April in the afternoon, the fun being bright, we fet fire to a fir plank at 150 feet distance, with only 128 glasses: the inflammation was very sudden, and made in all the extent of

the

he focus which was about fixteen inches diameter at his distance.

The same day, at half past two o'clock, we threw the fire on another plank, partly pitched and covered with sulphur in some places: the inflammation was made very suddenly; it began by the parts of the wood which were uncovered, and the fire was so violent, that the plank was obliged to be dipped in water to extinguish it: there were 148 glasses at 150 feet distance.

The 11th of April, the focus being only twenty feet distant from the mirror, it only required twelve glaffes to inflame small combustible matters: with twenty-one glasses we set fire to another plank which had already been partly burnt; with forty-five glasses we melted a block of tin of 6lb. weight; and with 117 glaffes we melted thin pieces of filver, and reddened an iron plate. I am also persuaded, that at fifty feet we shall be able to melt metals as well as at twenty, by using all the glasses of the mirror; and as the focus at this distance is fix or feven inches broad, we shall be able to make trials on all metals, which it was not possible to do with common mirfors, whose focus is either very weak, or 100 times fmaller than that of my mirror. I have remarked, that metals, and especially filver, smoke much before they melt: the smoke was so striking, that it shaded the ground, and it was there I looked on it attentively; for it is not possible to look a moment on the focus when it falls on the metal; the luftre is much more dazzling than that of the fun.

The experiments which I have here related, have been followed by a great number of experiments which confirm them. I have fet fire to wood at 210 feet distance with this mirror, by the sun in the summer; and I am certain, that with four similar mirrors I could burn at 400 feet, and, perhaps, at a greater distance. I have likewise melted all metals, Vol. V. A a and

and metallic minerals, at 125, 30, and 40 feet. We shall find, in the course of this article, the uses to which these mirrors can be applied, and the limits we must assign to their power for calcination, com-

bustion, fusion, &c.

This mirror burnt according to the different inclination given it, and what gave it this advantage over the common reflecting mirrors was, that its focus was very diffant, and had so little curvature, that it was almost imperceptible: it was seven feet broad by eight feet high, which makes about 150th part of the circumference of the sphere, when we burn at

150 feet distance.

The reason that determined me to prefer glasses of fix inches broad by eight inches high, to square glasses of fix or eight inches, was, that it is much more commodious to make experiments upon an horizontal and level ground, than otherwise; and that with this figure higher than broader, the images were rounder; whereas, that with square glasses, they would be shortened, especially at small distances, in this horizontal situation.

This discovery furnishes us with many useful things for physic, and, perhaps, for the arts. We know, that what renders the common reflecting mirrors almost useless for experiments, is, that they burn almost always upwards, and that we are greatly embarraffed to find means to suppress or support matters to be melted or calcined to their focus. means of my mirror, we burn concave mirrors downwards, and with fo great an advantage, that we have what degree of heat we please: for example, by oppofing to my mirror, a concave one of a foot fquare in the furface, the heat produced to this last mirror. by using 154 glasses only, will be upwards of twelve times greater than that generally produced, and the effect will be the same as if twelve suns existed inflead flead of one, or rather as if the fun had twelve times more heat.

Secondly, By means of my mirror, we shall have the true scale of the augmentation of heat, and make a real thermometer, whose divisions will be no longer arbitrary, from the temperature of the air to what degree of heat we chuse, by letting fall, successively, the images of the sun one on the other, and by graduating the intervals, whether by means of an expansive liquor, or a machine of dilatation; and from that we shall know, in fact, what a double, treble, quadruple, &c. augmentation of heat is, and shall find out matters whose expansion, or other effects, will be the most suitable to measure the augmentations of heat.

Thirdly, We shall exactly know how many times is required for the heat of the fun to burn, melt, or calcine different matters, which was hitherto only known in a vague and very indefinite manner, and thall be in a state to make precise comparisons of the activity of our fires with that of the fun, and have exact relations, and fixed and invariable measures. In thort, we shall be convinced when we examine my theory, and shall have feen the effect of my mirror, that the mode I have used, was the only one possible to fucceed to burn far off: for, independently of the phyfical difficulty of making large concave, fpherical, parybolical mirrors, or of any other curvature whatfoever, regular enough to burn at 150 feet distance. we shall easily be convinced, that they would not produce but nearly as much effect as mine, because the focus would be almost as broad: that besides, thefe curved mirrors, if even it should be possible to make them, would have the very great difadvantage to burn only at a nigh distance, whereas, mine burns at all diffances; and, confequently, we shall abandon the scheme of making mirrors to burn at a great distance by means of curves, which has uselessly employed

ployed a great number of mathematicians and artists, who were always deceived; because, they considered the rays of the sun as parallel, whereas, they should be considered as they are, i. e. as forming angles of all sizes from 0 to 32 minutes, which makes it impossible, whatsoever curve is given to a mirror, to render the diameter of the socus smaller than the cord, which measures 32 minutes: thus, if even we could make a concave mirror to burn at a great distance; for example, at 150 feet, by employing all its points on a sphere of 600 feet diameter, and by employing an uncommon mass of glass or metal, it is evident, that we shall have nearly no more advantage than by using, on the contrary, only small plain mirrors.

On the whole, as every thing has its limits, and although mine is susceptible of a very great perfection, as well for the adjustment, as for many other things; and, as I think I shall be able to make another whose effects will be superior, nevertheless, it cannot be expected ever to burn at extreme distances; to burn, for example, at the distance of half a mile, a mirror would be required 2000 times larger; and all that can ever be effected, is to burn at the distance of 8 or 900 feet. The focus, whose motion is always correspondent to that of the sun, moves so much the quicker, as it is farther distant from the mirror; and at 90 feet, it would move about fix feet a minute.

However, as I have given an account of my discovery, and the success of my experiments; I should render to Archimedes and the ancients the glory that is their due. It is certain, that Archimedes could perform with metal mirrors what I have done with glass, and that, consequently, I cannot resuse him the title of the first inventor of these mirrors, which the opportunity he had to use them, rendered him, without doubt, more celebrated than the merit of the thing itself.

Many

Many advantages may be derived from the use of these mirrors, with an assemblage of small mirrors, with hexagonal planes, and polished steel, which will have more solidity than glasses, and which would not be subject to the alterations which the light of the sun may cause, we may produce very useful effects, and which would amply repay the expences of the construction of the mirror.

1. For all evaporations of falt waters, where great quantities of wood and coal are confumed, or ftructures for the purpose of carrying the waters off, which cost more than the construction of many mirrors, fuch as I mention: for the evaporation of falt waters, only an affemblage of twelve plain mirrors of a fquare foot each is necessary. The heat reflected by their focus, although directed below their level, and at fifteen or fixteen feet distance, will be still great enough to boil water, and, confequently, produce a quick evaporation; for the heat of boiling water is only treble the heat of the fun in fummer; and as the reflection of a well polished plain surface only diminishes the heat one half, fix mirrors are only required to produce to the focus a heat equal to boiling water; but I shall double the number to make the heat communicate quicker; and likewife, by reason of the loss occasioned by the obliquity, under which the light falls on the furface of the water to be evaporated, and because falt water heats flower than fresh. mirror, whose affemblage would form only a fquare four feet broad by three feet high, would be eafy to be managed; and if we defired to double or treble the effects in the fame time, it would be better worth while to make many fimilar mirrors, i.e. to double or treble the number of these mirrors of four feet by three, than to augment the extent of them; for water can only receive a certain quantity of heat, and we should not gain any thing by increasing this degree; whereas, by making two focus's by two equal mirrors, we shall double the effect of the evaporation, and we shall treble it by three mirrors, whose focus's will fall separately one from the other on the surface of the water to be evaporated. On the whole, we cannot avoid the loss caused by the obliquity; and if we would remedy it, it can only be done by another still greater, by receiving the rays of the sun on a great glass which would reflect them broken on the mirror; for then it would burn at bottom instead of the top, but it would lose half the heat by the first reflection, and half of the remainder by the second; insomuch, that instead of fix small mirrors, it would require a dozen to obtain a heat equal to boiling water.

For the evaporation to be made with more fuccess, we ought to diminish the thickness of the water as much as possible: a mass of water a foot deep will not evaporate so quick by a great deal, as the same mass reduced to six inches, and increased to double the superficies. Besides, the bottom being nearer the surface, it heats quicker, and this heat, which the bottom of the vessel receives, contributes still more to the

celerity of the evaporation.

2. These mirrors may be used with advantage to calcine plaisters, and even calcareous stones; but they would require to be larger, and the matters placed in an elevated fituation, that nothing may be loft by the obliquity of the light. It has before been observed, that gypsums heats as soon again as soft calcareous flone, and nearly twice as quick as marble, or hard calcareous stone; their calcination, therefore, must be in a respective ratio. I have found by an experiment repeated three times, that a little more heat is required to calcine white gypfum, called alabafter, than to melt lead. Now, the heat necessary to melt lead, is, according to Newton, eight times fronger than the heat of the summer sun: it, therefore, would require fixteen small mirrors to calcine gypfum; gypfum; and because of the losses thereby occasioned. as well as by the obliquity of the light as the inequality of the focus, that is not removed above fifteen feet, I prefume, it would require twenty, and perhaps twenty-four mirrors of a foot square each, to calcine gypfum in a fhort time; confequently, it would require an affemblage of forty-eight small mirrors to calcine the foftest calcareous stone, and seventytwo of a foot square to calcine hard calcareous stones. Now, a mirror twelve feet broad by fix feet high, would be a large and cumbersome machine: neverthelefs, we should conquerthese difficulties, if the product of the calcination was confiderable enough to be equivalent, and to furpass the expence of the confumption of wood. To afcertain this, we ought to begin with calcining of plaister with a mirror of twenty-four pieces, and if that fucceeded, to make two other like mirrors, instead of making a large one of feventy-two pieces; for by coinciding the focus's of these three mirrors of twenty-four pieces, we shall produce an equal heat, strong enough to calcine marble or hard stone.

But a very effential matter remains doubtful, that is, to know how long time would be requifite to calcine a cubical foot of matter, especially if that foot was struck with the heat only in one part? Some time would pass before the heat penetrated its thickness; that, during this time, a very great part would be lost, and which would issue from this piece of matter after it had entered therein. I greatly doubt, therefore, that the stone not being touched by the heat on every side at once, the calcination would be slower, and the produce less. Experience alone can decide this; but it would be at least necessary to attempt iton gypsous matters, whose calcination is as quick again as calcareous stone.

By concentrating this heat of the fun in a kiln, which has no other opening than that which admits

the light, a great part of the heat would be prevented from flying off, and by mixing with calcareous stone a fmall quantity of brark, or coal-dust, which is the cheapest of all combustible matters: this slight quantity of food would fuffice to feed and augment the quantity of heat, which would produce a more ample and quick calcination, and at very little ex-

pence.

3. These mirrors of Archimedes may be, in fact, used to set fire to the fails of vessels, and even in the pitched wood at more than 150 feet distant : it might also be used against the enemy, by burning the grain and other productions of the earth: this effect, which would be fudden, would be very destructive; but we shall not dwell on the means of doing mischief, think on those which may do some real service to

human nature:

4. These mirrors furnish the sole means of exactly measuring heat. It is evident, that two mirrors. whose luminous images unite, produce double heat in all the points of its furface: that three, four, five, or more mirrors will also give a treble quadruple, quintuple, &c. heat; and that, confequently, by this mode we can make a thermometer whose divisions will not be too arbitrary, and the scales different; like those of the present thermometers. The only arbitrary thing which enters into the composition of this thermometer, would be the supposition of the total number of the parts of the quickfilver by quitting the degree of absolute cold; but by taking it to 10000 below the congelation of water, instead of 1000, as in our common thermometers, we should approach greatly towards reality, especially by chufing the coldest days in winter to mark the thermometers: every image of the fun would give it a degree of heat above the temperature of ice. The point to which the mercury rifes by the first image of the fun, would be marked I, and fo fo on to the highest, which might be extended to 36 degrees. At this degree we should have an augmentation of heat, thirty-fix times greater than that of the first, eighteen times greater than the fecond, twelve times greater than the third, nine times greater than the fourth, and so on; this augmentation of thirtyfix of heat above that of ice would be great enough to melt lead; and there is every appearance to think that mercury, which volatilizes by a much less hear. would by its vapour break the thermometer. We cannot, therefore, extend the division farther than twelve, and perhaps not farther than nine degrees, if mercury is used for these thermometers, and by this means we shall have only nine degrees of the aug+ mentation of heat. This is one of the reasons which induced Newton to make use of linseed oil instead of quickfilver; and, in fact, we can, by making use of this liquor, extend the division not only to twelve degrees, but as far as to make this oil to boil. I do not propose to fill the thermometers with closed spirits of wine: it is univerfally known, that this liquor decomposes in a very short time, and that it cannot be used for experiments of a strong heat.

When on the scale of these thermometers filled will oil or mercury, the first divisions 1, 2, 3, 4, &c. are marked to indicate the double, treble, quadruple, &c. augmentations of heat, we must search after the aliquot parts of each division; for example of the points $1\frac{1}{4}$, $2\frac{1}{4}$, $3\frac{1}{4}$, &c. or $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c. and $1\frac{3}{4}$, $2\frac{3}{4}$, $3\frac{3}{4}$, and which will be obtained in an easy manner, by covering the $\frac{1}{4}$, $\frac{1}{2}$, or 2-4ths of the superficies of one of those small mirrors; for then the image which it restects, will contain only the $\frac{1}{4}$, $\frac{1}{2}$, or $\frac{3}{4}$ of the heat which the whole image will contain, and, consequently, the division of the aliquot parts will be

as exact as those of the whole numbers.

If once we fucceed in this real thermometer, which I call real, because it really marks the proportion of Vol. V. Bb the

the heat, every other thermometer whose scale is arbitrary and different, will become not only superfluous, but even hurtful in many cases, to the precision of physical truths sought after by their means.

5. By means of three mirrors we may eafily collect in their purity, the volatile parts of gold, filver, and other metals and minerals; for, by exposing to the large focus of these mirrors a great plate of metal, as a dish, or filver plate, we shall see a smoke issue therefrom in great abundance, and for a considerable time, till the metal is in susion, and by giving only a smaller heat than what susion requires, we shall evaporate the metal so as to diminish the weight consi-

derably.

I am certain of this first circumstance, which surnish lights on the intimate composition of metals: I was desirous of collecting this plentisul vapour which the pure fire of the sun causes to issue from the metal; but, I had not necessary instruments, and I can only recommend to chemists and physicians, to follow this important experiment, the results of which would be as much less equivocal as the metallic vapour is pure; whereas, in all like operations made with common fire, the metallic vapour is necessarily mixed with other vapours proceeding from combustible matters which serve for food to this fire.

Besides, this means, is the only one we have to volatilize fixed metals, such as gold and silver; for I presume, that this vapour which I have seen raise in such great quantities from these fixed metals heated in the large socus of my mirror, is neither of water, nor of any other liquor, but of the parts even of the metal which the heat detaches by volatilizing them. By receiving these vapours of different metals, mix them together, and by this mode make more intimate and pure alloys, than is made by susion and the mixture of these metals when melted, which never perfectly unite by reason of the inequality of their specifical

fical weight, and of many other circumstances, which are opposed to the intimate and perfect equality of the mixture. As the constituent parts of these metallic vapours are in a much greater state of division than suffice, they would join and unite closer and more readily. In short, we should attain the knowledge of a general sact by this mode, and which for many reasons I have a long time suspected, that there is penetration in all alloys made in this manner, and that their specific weight would be always greater than the sum of the specific weights of the matters of which they would be composed: for penetration is only a greater degree of intimity, every thing equal in other respects will be so much the greater as matters will be in a more

perfect state of division.

By reflecting on the veffels used to receive and collect these metallic vapours, I was furnished with an idea, which appeared to me to be of too great utility not to publish; it is also easy enough to be realized by good able chemists. I even communicated it to some of them, who appeared quite satisfied with it. This idea is to freeze mercury in this climate, and with a much less degree of cold than that of the experiments of Petersbourg or Siberia. For this purpose, the vapour of mercury is only required to be received, and which is the mercury itself volatilized by a very moderate heat in a crucible, or veffel, to which we give a certain degree of artificial cold. This vapour, i. e. this mercury minutely divided will offer to the action of the cold furfaces fo large, and maffes fo small, that instead of 187 degrees of cold requisite to freeze mercury, perhaps only 18 or 20 will be necessary, and, perhaps, even less to freeze it when in vapour. I recommend this important experiment to all those who endeavour earnestly for the advancement of the sciences.

To these principal uses of the mirror of Archimedes, I could add many other particular ones; but I have confined myself to those only which have appeared the most useful, and least difficult to be put in practice. Nevertheless, I thought it my duty to subjoin some experiments that I made on the transmission of light through transparent bodies; and, at the same time, to give some new ideas on the means of seeing objects at a distance with the naked eye, or with a mirror, like that spoken of by the ancients, and by the effect of which, vessels could be perceived from the port of Alexandria, as far as the curvature of the earth would permit.

Physicians at present know, that there are three causes which prevent the light from uniting in a point, when its rays have passed the objective glass of a common mirror. The first, is, the spherical curve of this glass, which disperses a part of the rays in a space terminated by a curve. The second, is the angle under which the object appears to the naked eye; for the breadth of the socus of the objective glass has a diameter nearly equal to the chord of which this angle measures. The third, is the disferent refrangibility of the light; for the most refrangible rays do not collect in the same place where

the leffer refrangible rays do.

The first cause, may be remedied by substituting, as Descartes has proposed, elliptical or hyperbolical glasses to the spherical. The second is to be remedied by a second glass, placed to the socus of the objective, whose diameter is nearly equal the breadth of this socus, and whose surface is worked on a sphere of a very short ray. The third has been found to be remedied, by making telescopes, called Acromatics, which are composed of two sorts of glasses which disperse the coloured rays differently; so that the dispersion of the one is corrected by the other, without the general refraction, which constitutes

feet long, made on this principle, is in effect equiva-

lent to the old telescopes of 25 feet.

On the whole, the remedy of the first cause is perfectly useless to this present time, because the effect of the last being much more considerable, has such great influence on the whole effect, that northing can be gained to substitute hyperbolical or elliptical glasses to spherical, and that this substitution could not become advantageous, but in the case where the means of correcting the effect of the different refrangibility of the rays of light might be found: it seems, therefore, at present, that we should do well to combine the two means, and to substitute, in

acromatic telescopes, elliptical glasses.

To render this more obvious, let us suppose, the object observed to be a luminous point without extent, as a fixed star is to us, It is certain, that with an objective glass, for example, of thirty feet focus. all the images of this luminous point will extend in the form of a curve to this focus, if it is worked on a fphere; and, on the contrary, they will unite in one point, if this glass is hyperbolical: but, if the object observed at a certain extent, as the moon which occupies half a degree of space to our eyes. then the image of this object will occupy a space of three inches diameter in the focus of the objective glass of thirty feet; and the abercation caused by the fphericity, producing a confusion in any luminous point, it produces the fame on every luminous point of the moon's disk, and, consequently, wholly dif-There would be, then, much disadvanfigure it. tage in every case to make use of elliptical glasses for long telescopes, fince the means has been found in a great measure to correct the effect produced by the different refrangibility of the mirrors.

From what we have observed, it follows, that if we would make a telescope of thirty feet to observe

the moon with, and fee it completely, the ocular glass must be at least three inches diameter to collect the whole image, which the objective glass produces to its focus; and if we would observe this planet with a telescope of fixty feet, the ocular glass must be at least fix inches diameter; because the chord which the angle measures, under which the moon appears to us, is, in this case, three inches, and nearly fix inches: therefore, aftronomers never make use of telescopes which include the whole disk of the moon, because they would not magnify but very little. But, if we would observe Venus with a telescope of fixty feet, as the angle under which it appears to us is only fixty feconds, the ocular glass can only have four lines diameter; and if we make use of an objective of 120 feet, an ocular glass of eight lines diameter would suffice to unite the whole image which the objective forms to its focus.

Hence we see, that if even the rays of light were equally refrangible, we could not make such strong telescopes to see the moon with, as to see the other planets, and that the smaller a planet appears to our sight, the more we can augment the length of the telescope with which we can see it wholly. Hence it may be well conceived, that in this supposition of the rays equally refrangible, there must be a certain length more advantageously determined than any other for such or such planet, and that this length of the telescopes, not only depend, on the angle under which the planet appears to our sight; but also, on the quantity of light with which it is

brightened.

In common telescopes, the rays of light being differently refrangible, all that can be done in this mode to perfectionate them, would not be very advantageous; because, that under whatsoever angle the object or planet appears to our sight, and whatever intensity of light it may have, the rays

will

will never collect in the fame part; the longer the telescope is, the more interval it will have between the focus of the red and violet rays, and consequently the more confused the image of the object observed.

Refracting telescopes therefore cannot be perfectionated, but by feeking for the means of correcting this effect of the different refrangibility, either by composing telescopes of different densities; or by other particular means, which would be different according to different objects and circumstances. Suppose, for example, a short telescope composed of two glasses, one convex and the other concave, it is certain, that this telescope can reduce itself to another, whose two glasses would be plain on one fide, and on the other bordering on spheres, whose rays would be shorter than that on the spheres on which the glaffes of the first telescope have been constructed. At prefent to avoid this, a great part of the effect of the different refrangibility of the rays, this fecond telescope may be made with one fingle piece of massive glass, as I had it done with two pieces of white glass, one of two inches and an half in length, and the other one inch and an half, but then the loss of transparency is a greater inconvenience than that of the different refrangibility corrected by this means, for these two small massive telescopes of glass, are more obscure than a small common telescope of the same glass and dimensions. They indeed give less iris, but are not better, and they are made longer: in massive glass, the light after having croffed this thickness of glass, would no longer have a fufficient force to take in the image of the object to our eye. So to make telefcopes ten or twenty foot long, I only find water that has sufficient transparency to suffer the light to pass without entirely extinguishing it in this great thick-By using therefore water to fill up the inter-

vals between the objective and the ocular glass, we shall in part diminish the effect of the different refrangibility; because, that water approaches nearer to glass than air, and if we could, by loading the water with different falts, give it the fame refringent degree of power as to glass; it is not to be doubted, that we should correct still more by this means the different refrangibility of the rays: a transparent liquor should be therefore used, which would have nearly the same refrangible power as glass; for then it would be certain, that the two glasses with their liquor between them, will in part correct the effect of the different refrangibility of the rays, in the fame mode as it is corrected in the small massive telescope which I speak of.

According to the experiments of M. Bougues, the thickness of a line of glass destroys - of light, and confequently the diminution would be made in

the following proportion.

Thickness, 1, 2, 3, 4, 5, 6 times.

Diminution $\frac{2}{7} \frac{10}{47} \frac{50}{343} \frac{250}{2401} \frac{1250}{16807} \frac{6*50}{117649}$ infomuch, that by the sum of these fix terms, we should find, that the light which passes through fix lines of glass, would have already lost 102034 that is, about 10 of its quantity. But it must be considered, that M. Bouguer makes use of glasses which are but little transparent, fince he has observed, that the thickness of a line of these glasses destroys - of the light. By the experiments which I have made on different kinds of white glass, it has appeared to me that the light ' These experiments are easy diminishes much less. to be made, and what all the world may repeat.

In a dark chamber whose walls were blackened, and which I made use of for optical experiments, I had a candle lighted of fixes to the pound, the room was very large, and the candle the only light in it.

The

I then tried at what distance I could read by this light, and found that I read very eafily at twenty-four feet four inches from the candle. Afterwards, having placed a piece of glass about a line thick before it, at two inches distance, I found that I still read very plainly at twenty-two feet nine inches: and by fubstituting to this glass another piece of two lines in thickness, and of the same glass, I read at twenty-one feet distance from the candle. Two of the same glaffes joined one to the other, and placed before the candle, diminished the light so much, that I could only read at seventeen seet and an half distance: and at length, with three glasses, I could only read at fifteen feet. Now, the light of a candle diminishing as the square of the distance augments, its diminution should have been in the following progression, if glaffes had not been interposed,

Therefore, the losses of the light, by the interposition of the glasses, are in the following progression, $84\frac{79}{144}$. $151.\ 285\frac{7}{9}$. $367\frac{1}{4}$.

From hence it may be concluded, that the thickness of a line of this glass diminishes only $\frac{84}{592}$ of light, or about $\frac{1}{7}$: that two lines diminishes $\frac{157}{592}$, not quite $\frac{1}{4}$, and three glasses of two lines, $\frac{367}{592}$, i. e. less than $\frac{2}{3}$

than $\frac{2}{3}$.

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As this result is very different from that of M. Bouguero, and as I was cautious of suspecting the truth of his experiments, I repeated mine with common glass. For long telescopes water can alone be used; and, it is still to be feared that an inconveniency will subsist; for what will be the opacity resulting from this quantity of liquor which, I suppose, fills the interval between the two glasses?

The longer the telescope, the greater loss of light will ensue; so that it appears at first fight, that this mode cannot be used, especially for long telescopes; for following what M. Bouguer says in his Optical Essay, on the gradation of light, nine seet seven inches sea water, diminishes the light in a relation of 14 to 5; therefore, these long telescopes filled with water, can not be used for observing the sun, and the other stars would not have light enough for to perceive them across a thickness of twenty or

thirty feet of intermediate liquor.

Nevertheless, if we consider, that by allowing only an inch, or an inch and an half, for the bore of an objective of thirty seet, we shall very distinctly perceive the planets in the common telescopes of this length; we may suppose, that by allowing a greater diameter to the objective, we should augment the quantity of light in the ratio of the square of this diameter; and, consequently, if an inch bore suffices to see a star distinctly in a common telescope, three inches bore will be sufficient to see it distinctly through a thickness of ten seet water; and that with a glass of three inches diameter, we should equally see it through a thickness of twenty feet water, and so on.

It appears, therefore, that we might hope to meet with success in constructing a telescope on these principles; for, by increasing the diameter of the objective, we partly regain the light lost by the defect of

the transparency of the liquor.

But what appears to me certain, is, that a telefcope constructed on this mode, would be very useful to observe the sun; for, by supposing it even the length of 100 feet, the light of that planet would be not too strong after having traversed this thickness of water, and we should observe the surface of this planet, leisurely and easily, without the need of making use of smoked glasses, or of receiving the image on pasteboard; an advantage no other tele-

scope can have.

There would be only fome trifling difference in the construction of this solar telescope, if we wanted the whole face of the fun presented; for, supposing it the length of 100 feet, in this case, the ocular glass would require to be ten inches diameter; because the fun, taking up more than half a celestial degree, the image formed by the objective to its focus at 100 feet, will at least have this length of ten inches; and to unite it wholly, it will require an ocular glass of this breadth, to which only twenty inches of focus should be given to render it as strong as possible: for the objective, as well as the ocular glass, should be ten inches diameter, in order that the image of the planet, and the image of the bore of the telescope, should be of an equal fize with the focus.

If this telescope which I propose, should only ferve to observe the sun exactly, it would be of great service: for example, it would be very curious to be able to discover whether there are greater or less luminous parts than others in the sun; if there are inequalities on its furface, and of what kind; if the spots float on its surface; or whether they are fixed there, &c. The brightness of its light prevents us from observing it with the naked eye, and the different refrangibility of its rays renders its image confused when received in the focus of an objective glass, or on pasteboard, so the surface of the fun is less known to us, than that of other pla-This different refrangibility of its rays would not be near entirely corrected in this long telescope filled with water; but if this liquor could, by the addition of falts, be rendered as dense as glass, it would then be the fame as if there was only one glass to pass through; and it appears, that there would be more advantage in making use of these

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Be it as it may, this is certain, that to observe the fun, a telescope is required quite different from those we make use of for the other planets; and it is also very certain, that a particular telescope is necessary for each planet, proportionate to their intensity of light, i. e. to the real quantity of light with which they appear to be enlightened. In all telescopes the objectives are required as large, and the ocular glass as firong, as possible, and, at the same time, the distance of the focus proportionated to the intensity of the light of each planet. To do this with the greatest advantage, it is requisite to use only an objective glass so much the larger, and a focus so much the shorter as the planet has light. Why has there not hitherto objective glasses been made of 243 feet diameter? The aberration of the rays caused by the fphericity of the glaffes, is the fole cause of the confusion, which is as the square of the diameter of the tube; and it is for this reason that spherical glaffes with a fmall bore are of no value when enlarged; we have more light, but less distinction and clearness. Nevertheless, broad spherical glasses are very good for night telescopes: the English have constructed telescopes of this nature, and they make use of them very advantageously to see vessels at a great distance in dark nights. But at present, as we know how, in a great measure, to correct the effects of the different refrangibility of the rays, it feems, that we should make elliptical or hyperbolical glasses, which would not produce this alteration caused by fphericity, and which, confequently, would be three or four times broader than spherical glasses. There is only this mode of augmenting to our fight the quantity of light fent to us from the planets, for we cannot put an additional light on the planets, as we do on objects we observe with the microscope, but must must at least employ to the greatest advantage possible, the quantity of light with which they are illumined, by receiving it on as great a surface as possible. This hyperbolical telescope, which would be composed only of one single large objective glass, and of an occular one proportionate, would require matter of the greatest transparency. We should unite by this means all the advantages possible, that is to say, those of the acromatic to that of the elliptical or hyperbolical telescopes, and we should profit of all the quantity of light each planet reslects to our sight. I may be deceived, but what I propose appears to be sufficiently sounded to recommend its execution to persons zealously attached to the advancement of the sciences.

Employing myself thus on these reveries, some of which may one day or other be realized, and on which hope I publish them, I thought of the Alexandrian mirror, spoken of by some ancient authors, and by means of which vessels were seen at a great distance on the sea. The most positive passage I met with is the following;

"Alexandria....in Pharo vero erat speculum e ferro sinico. Per quod a longe videbantur naves Græcorum advenientes; sed paulo postquam Isla-" mismus invaluit, scilicet tempore califatus Walidsil: Abdi-l-melec, Christiani, fraude adhibita illud deleverunt. Abu-l-feda, &c. Descriptio Ægypti."

I have imagined, 1. That such a mirror was possible to be made. 2. That even without a mirror or telescope, we might by certain dispositions obtain the same effect, and see vessels from land, as far, perhaps, as the curvature of the earth would permit. We have observed that persons whose sight was very good, have perceived objects illumined by the sun, at more than 3400 times their diameter, and at the same time we have remarked, that the intermediate light was of such great hurt to that of distant objects,

jects, that by night a luminous object is perceived at ten, twenty, and perhaps a hundred times greater distance than during the day. We know that at the bottom of very deep pits, stars may be seen in the day-time; why therefore should we not see vessels illumined by the rays of the fun, by placing one's felf at the end of a very long, dark gallery, fituated on the fea shore, in such a manner as to receive no other light than that of the distant sea, and the vesfels which might be thereon. This gallery is only an horizontal pit which has the same effect with respect to ships as the vertical pit with respect to the stars; and this appeared to me so simple, that I am aftonished it has never before been thought of. It feems to me, that by taking the time of the day for our observations when the fun should be behind the gallery, that is to fay, the time when the veffels would be much illumined, we might fee them from the bottom of this dark gallery, ten times at least better than in the open light. Now, a man on horse is eafily distinguished at a mile distance, when the rays of the fun shines on them, and by suppressing the intermediate light which furrounds us, and darkening our fight, we shall see at least ten times farther; that is to fay, ten miles. Ships would therefore be feen much larger, and as far as the curvature of the earth would permit, without any other instrument than the naked eye.

But a concave mirror of a great diameter and any focus, placed at the end of a long black tube, would have nearly the same effect as our great objective glasses of the same diameter and form would have during the night, and it was probably one of these concave mirrors of polished steel, that was established at the port of Alexandria. If this steel mirror has really existed, we cannot refuse to the ancients the glory of the first invention; for this mirror can be only effected by as much as the light reslected by its sur-

face

face, was collected by another concave mirror placed at its focus, and in this confifts the essence of the telescope and the facility of its construction. Nevertheless, this does not deprive Newton of any glory, who first renewed the almost forgotten invention. As the rays of light are by their nature differently refrangible, he was inclined to think there were no means of correcting this effect; or, if he had perceived these means, he judged them so difficult. that he chose rather to turn his views another way. and produce by means of the reflexion of the rays, the great effects which he could not obtain by their refraction. He has therefore constructed his telescope, the reflection of which is much superior to that of common telescopes. The best telescope's are always dark in comparison of the acromatic, and this obfcurity does not proceed only from the defect of the polish or the colour of the metal of mirrors, but from the nature even of light, the rays of which being differently refrangible, are also differently reflexible, although in much less unequal degrees. It remains therefore to perfectionate the telescope as much as possible, and to find the manner of compensating this different reflexibility, as we have discovered that of compensating the different refrangibility.

After all that has been spoken of, I imagine that it will be well perceived, that a very good day glass may be made, without using either glasses or mirrors, and simply by suppressing the surrounding sight by means of a tube 150 or 250 feet long, and by placing ourselves in an obscure place. The brighter the day is, the greater will be the effect. I am persuaded that we should be able to see at sisteen and perhaps twenty miles distance. The only difference there is between this long tube and a dark gallery, which I proposed, is, that the field, i. e. the space seen would be smaller and precisely in the ratio

of the square of the bore of the tube to that of the gallery.

OBSERVATIONS and EXPERIMENTS, made with a View of improving CANNONS.

CANNONS are made of cast iron, in France as well as in England, Holland, and elsewhere. Two motives have given birth to this custom: the first is that of economy; for a cannon of cast iron costs much less than a cannon of forged iron, and still much less than a brass cannon; and that alone has, perhaps, been fufficient to give them the preference. It is likewise pretended, and I am inclined to believe it, that the brass cannon, some of which our veffels for parade are armed with, renders in explosion so violent a report, as to make the people in the veffel deaf for a short time. On the other hand, it is afferted, that the cannons of forged iron must not be used on board of ships by reason of their lightness, which one would imagine ought to make them preferred: the explosion causes them to recoil from the portholes wherein, it is faid, it is impossible to detain them firm, nor even sufficiently to give them a certain direction. If this inconvenience is not real, or if it can be remedied, no doubt but cannons of forged iron ought to be preferred to those of cast, as they would be as light again, and have more than double the refistance. Marshal Vauban fabricated fome very good ones, of which the remains are yet to be feen at the manufacture of Charleville. The labour would not be more difficult than that of anchors, and a manufactory, as well established for this purpose as that of M. la Chaussade for anchors, might be of great utility. However,

However, as this is not the actual state of things, we shall only carry our observations on cast iron cannons. Great complaint is made of their weak refistance in these latter times; for, in spite of the most nice proofs, some have split on board our vessels; a terrible accident, which never happens without doing great injury, and loss of many men. The minister, willing to remedy this evil, or rather prevent it for the future, being informed, that I made some experiments on cast iron, asked my advice thereon in 1768, and follicited me to employ my time on this important subject. I delivered myself up to it with zeal, and in concert with Viscount Morogues, a very enlightened gentleman, I gave some observations I made to the minister, with my experiments, and those which remained to be made to perfectionate cannons. I am yet ignorant of the refult and fuccefs. The minister for maritime affairs being changed, I no longer heard speak either of experiments or cannons: but, this ought not to prevent me from giving, without being asked, whatever I have found to be of use, during the three years I employed myfelf in this undertaking, and which shall compose the subject of this memoir.

Cannons melt in a perpendicular fituation, in molds of many feet depth, the breach downwards. As it requires many thousand weight of matter in fufion to make a large cannon filled and charged with the mass which must compress it in its superior part, it was a prejudiced opinion, that two and even three furnaces were necessary to melt a large cannon. As the largest sows of iron melted in the greatest furnaces, are only 2500, or at most 3000lb. and as the matter in fusion remains only twelve or fifteen hours in the crucible of the furnace, it was imagined, that double or treble this quantity of matter in fusion, which would be obliged to be left for thirty-fix or forty hours in the crucible before it was run, would VOL. V. Dd not not only destroy the crucible, but even the surnace, by its bubbling and explosion; by means of which the most prudent step was taken, and large cannons run, by successively running at the same time the cast from two or three furnaces, placed so that the three currents of the metal might arrive at the same time into the mould.

It does not require much reflection to shew that this practice is bad: it is impossible that the fused metal of each of these furnaces is in the same degree of heat and fluidity; confequently, the cannon is found composed of two or three different matters; infomuch, that many of its parts, and often an whole fide is found necessarily of a worse or weaker matter than the rest, which is the greatest of all inconveniencies, with respect to refistance, since, the effort of the powder acting equally on all fides, never fails of burfting or cracking in the weakest parts. I therefore was resolved to try, and, in fact, see whether there was any danger to keep a greater quantity of metal in fusion a longer time than common; but found, during thirty-fix hours fusion, neither any other explosion nor bubbling than what sometimes happens when crude matters falls into the crucible. I then let it flow, and it formed three fows, weighing together 4600lb. of a very good caft.

By a fecond experiment, I retained the metal forty-eight hours in fusion without inconvenience. This long stay only purified it the more, and, confequently, diminished the volume by increasing the mass. As it contained a great quantity of hetorogeneous parts, some of which burnt, and others converted into glass; one of the greatest means of depurating it, is to permit it to remain in the fur-

nace.

Being, therefore, well affured, that the prejudice of the necessity of two or three furnaces, was very badly founded, I proposed to reduce the furnaces of Rouelle, Ruelle, in Augoumois, to one, which was followed and executed by the order of the minister: twenty-four pounders were melted without inconvenience, and with success, in a single surnace; and I have all the reason imaginable to presume, that thirty-six pounders might be melted, and would succeed. This first point obtained, I sought whether there were not still other causes which might contribute to the fragility of our cannon; and, in sact, sound some which contributed still more than the inequality of the stuff of which they were composed, by

running them from two or three furnaces.

The first of those causes, is the bad custom established for upwards of twenty years, to turn the external furface of the cannons to render them more agreeable to the eye; nevertheless, it is with the cannons, as with the foldier, he should be rather robust than elegant; and these cannons, turned, and polished, ought not to impose on the eyes of our brave sea officers: for, I think it demonstrable, that they are not only much weaker, but also of a less duration. The little we are versed in the knowledge of the fusion of iron ore, we have remarked by anvils, balls, &c. and with greater reason cannon, that the centrifrugal force of the heat impels the most massive and the purest part to the circumference; there remains then only to the center what is the worst, and often even there is formed a cavity. Among a number of bullets which were broken, more than half were found to have a cavity in their center, and in all the rest a matter more porous than the other part of the bullet. We shall farther remark, that there are many rays tending from the center to the circumference, and that the matter is more compact, and of a better quality, in proportion as it is more remote from the center. We shall likewise observe, that the surface of the bullet, mortar, or cannon, is much harder than the internal

part: this greater hardness proceeds from the moisture of the mould to the external part of the piece, which penetrates to the thickness of three lines in small pieces, and to a line and an half in large. is in this the greatest strength of the cannon confists; for this external layer unites the extremities of all the diverging rays I speak of, which are the lines by which the rupture is made; it ferves as armour to the cannon, is the purest part, and by its great hardness, contains all the internal parts which are softer, and would cede more eafily without that to the force of the explosion. Now, what is done in the turning of cannons? We begin, by raifing up by instruments all that external furface; we penetrate into the external part of the piece, to the point where it is found foft enough to permit itself to be turned, and by this operation take off, perhaps, one fourth of its force.

This external coat, which we are fo wrong in taking off, is, at the same time, the armour and fhield of the cannon: it not only gives it all the power of refistance it ought to have, but it likewise defends it from the rust which the turned cannons gather in a short time: we had better polish them with oil, or paint them. As the matter of the external furface is as tender as all the rest, the rust eats into it with a great deal more advantage, than in those whose furface is warranted by the temper. When I was, therefore, convinced, by my own observations, of the prejudice this mal-practice did to our cannons, I gave my advice to the minister to have it prescribed; but I do not think my advice was followed, because there were several persons very enlightened otherwise, and particularly M. de Moroques, who have thought differently. Their opinion, so contrary to mine, is founded on the temper rendering the iron more brittle; and hence, they looked on the external coat as the weakest and least refisting refisting of all the parts of the piece; and conclude, that not much injury is done by taking it off. They add, if we would remedy this evil, we have only to give a few lines more thickness to the cannon.

I own, that I could not give way to these reasons, We must distinguish in the temper, as in every thing elfe, many flakes, and even many links of it, Iron and steel heated white, and fuddenly dipped in very cold water, become very brittle: dipped in water not fo cold, they are much less brittle; and in hot water, the dipping does not give them any fenfible fragility. I have fome experiments hereon, which appear decifive. During the fummer, 1772, I dipped in river water, which was then warm enough to bathe in, all the iron bars, forged in one of the fires of my forge; and, comparing this iron with that which was not dipped, the difference of the grain was not fenfible, no more than that of their refistance to the mass when they are broken. But this iron, worked in the fame manner by the fame workmen, and dipped in the winter in the water of the same river which was almost frozen over, it not only became brittle, but, at the same time, lost all its nerve, fo that it might be thought to be no longer the fame iron. Now, the dipping, which is made to the furface of the cannon, is not certainly a cold one, it is produced only by the little humidity which iffues from the mould already dried. We must not, therefore, reason on it as on another cold tempering, nor conclude that it renders this external coat much more brittle than it would be without it. I fuppress many other reasons which I might alledge, because the thing appears sufficiently evident and clear.

Another object on which it is not so easy to pronounce affirmatively, is the practice of running the cannon solid, and to bore them afterwards with machines, difficult to be made, and still more difficult

cult to be conducted, instead of casting them hollow, as formerly, when our cannon split less than they do at present. I have weighed the reasons for and against, and shall here present them. To cast a cannon hollow, a core must be formed in the mould, and placed with the greatest exactness, that the cannon may be of a requifite thickness, and that one fide may not be stronger than another. As the matter in fusion falls between the core and the mould, it has much less centrifugal force, and hence the quality of the matter is not so unequal in the cannon cast hollow, as in that cast folid: but this matter also, for the reason that it is not so unequal, is in the whole not so good in the hollow cannon, because the impurities it contains is mixed therewith throughout, whereas in the other cannon, this bad matter rests in the center and is feperated afterwards from the cannon by boreing. I should suppose therefore, by this first reason, that the bored cannon ought to be preferred to the cored cannon. If we could nevertheless cast these cannon with fufficient precision, not to be obliged to touch the internal furface; if when we draw the core, this furface was fufficiently close, and equal in all its direction fo as not to have any need of calibration, and confequently partly destroyed by the steel instrument; they would have a great advantage over the other, because, in this case, the internal surface would be found like the external furface, and hence the refistance of the piece would be much greater. But our art does not extend fo far; we are obliged to scrape off the internal part of every piece cast hollow for calibration; by boring them we only do the fame, and we have the advantage of throwing out all the bad matter found around the centre of the piece cast folid: a matter, on the contrary, which remains dispersed over the whole mass of the piece cast hollow.

In other respects, cannon cast folid, are much less fubject to flaws, chambers, false soldering, &c. To cast cannons with a core well, and to render them perfect, vents are required; whereas, there is no need of them in the casting of them solid: as they do not touch the earth or fand of which their mould is composed but by the external surface; as it is rare that this mould is well prepared and dry; as somewhat may fall off, and that provided the cast is not made precipitately and very liquid, it retains neither the bubbles of air or water, which exhale in proportion as the mould fills; there must not arise so many defects by far in this matter cast solid, as in that where the core rendering to the internal part its air and humidity, can scarcely fail of occasioning blifters, flaws, and chambers, which will form fo much the easier, as the matter is less thick, its quality good and its refrigeration less sudden. Hitherto all things feem to concur in giving the preference to the practice of casting of cannons solid: nevertheless, as it requires less quantity of matter for hollow cannons, and that from thence it is more easy to depurate it in the furnace before it is run: that as the expence of the boring machines is immense, in comparison of those of cores, we should act right in trying whether, by the mode of vents, which I am going to propose, we might not attain to the pitch of rendering pieces cast with a core, perfect and rough, not to fear flaws, &c. and not to be obliged to raise up the temper of their internal furfaces: they would be then of a greater refistance than the others, which may be reproached with feveral defects which I am going to mention.

The more purified the cast is, the more compact, hard and difficult to bore will it be. The best steel tools will only cut it with difficulty, and the work will go on so much the slower as the cast is better. Those who introduced this practice, have therefore,

altered the nature of the matter for the convenience of their machines. They have changed the custom of making the cast hard, and have only run tender matter, which they have called douce, i.e. kind, that the difference may be the less perceptible. Hence all our cannon cast solid, have been fused from this kind matter, i. e. from a bad cast, and which has not near the purity, density, nor resistance which it ought to have. I acquired the compleatest proof of it by

the following experiments.

In 1767, I received from the forge of de la Nouce, in Brittany, fix large trunks of cannon cast folid. weighing together 5358 lb. The fummer following I had them brought to my forges, and having broken off the trunnions, I found the metal of a bad grain, which could not be discovered on the outside of the pieces, because emery or some other matter had been made use of, which filled the external pores. Having weighed this cast in the hydrostatical ballance, I found that it was too light, and weighed only 461 lb. the cubical foot; whereas, that run at my furnace weighed 504, and as I had purified it still more, it weighed 520 lb. the cubical foot. This fole proof might suffice to judge of this cast, but I shall not dwell on it. In 1770, I confructed a flove greater than my common floves, for the purpose of forging and converting into iron those cannon trunks, which I performed by means of wind and coals. I cast them into little fows, and after they were cooled, I examined the colour and grain by breaking the mass. I found, as I expected, the colour greyer and the grain finer. matter could not fail of being purer by this fecond fusion, and in fact, having tried it hydrostatically, I found it weighed 469 lb. the cubical foot; which nevertheless, does not come near the density requisite for a good cast. In short, I had no better success with the reft.

In 1770, I received from the forge at Rouelle in Angoumois, where the greatest part of our cannon are actually cast, famples of the cast from which they are run. It was of a grey colour, the grain fine, and the weight 495 lb. the cubical foot, reduced into forged iron with care. I found the grain like that of common iron, with little or no nerve, although worked in fmall rods and under the cylinder. So that this cast, although better than that which came to me from Rouelle is not good. I do not know whether their casts have been better and heavier fince that time; I only know, that two fea officers, very able and zealous men, have been fent thither, and are both able to perfectionate the art, and to conduct the works of this foundery proper. But till the epocha I mention, I am certain that the metal of our folid cannon, was but of a middling quality; that fuch a cast has not sufficient resistance, and that by depriving it also of the band which contains it, i. e. by taking off by the turning instruments, the furface, there is all the reason to fear the service of those cannon.

It may be here faid, that thefe are only ill-founded panicks, as cannons are never made use of before they have been proved, and that a piece once proved by one half more charge than common, cannot fly with the common charge. To this I answer, that this is not only uncertain, but the contrary is also much more probable. In general, the proof of cannon by powder is perhaps the worst method that can be used, to ascertain their refistance. Cannon cannot undergo the violent efforts of proofs without breaking, but by ceding as much as the co-herence of the matter permits, and as it requires that this matter of the cast is perfect spring, the parts seperated by a great effect cannot approach nor re-establish itself as they were at first. This cohesion of the integrant parts of the cast being therefore greatly diminished by the strong effort VOL. V.

of the proofs, it is not aftonishing that the cannon fplits afterwards with a common charge, it being a fimple effect derived from a cause as fimple. If the froke of the first proof divides the parts, a half or a third more than the common strokes, they will re-establish themselves and re-unite less in like proportion; for, although their coherence has not been destroyed, fince the piece refifted, it is not less true, that this coherence is not fo great as it was before, and as it has diminished in the same ratio as the strength of an imperfect fpring diminishes. Hence a second, or a third ftroke, will fplit the pieces which refifted at first, and those which have undergone the three proofs without breaking, are fcarcely fafer than the rest. After having undergone the fame misfortune, i. e. the too great seperation of their integrant parts, they neceffarily become weaker, and would confequently cede to the effort of the common charge.

A much furer mode, simple, and a thousand times cheaper to ascertain the resistance of the cannon, would be to weigh the cast hydrostatically, by putting a piece of the cast on one side when it is running, and if it weighs not at least when cold 520lb. the cubical foot, and they are not turned nor their surface touched, I dare affert that they would resist and endure as much as might be expected. I own, that by this mode, perhaps too simple to be adopted, it cannot be known whether the piece is sound or not, but once knowing the goodness of the cast, it would suffice to ascertain the rest, to try only once and by the common charge, cannons which are new, and we should be much surer of their resistance, than of those which

have undergone violent proofs.

Many persons have proposed improvements on cannons. Some have proposed to cover them with copper, others with forged iron, others solder this iron with cast, and all perhaps good in certain respects: in an art, the object of which is as important and the practice practice fo difficult, the efforts should be collected and the least discoveries recompensed. I shall not here make observations on the cannon of M. Feutry, which require a good deal of art in their execution, nor of some other ingenious attempts, but shall only observe, that the foldering of copper with iron renders it much more brittle: that when we solder cast iron with itself, by means of sulphur, we change its nature, and the line of the generation of these two soldered parts is no longer cast iron, but a very brittle pyrite. That in general, sulphur is an intermedium, which must never be used when we solder iron, without altering its quality, I only mention this for advice to those who might take this mode as the surest and easiest to render iron suspenses.

pieces.

If we retain the custom of boring of cannon, and if we cast them with good hard metal, it will be requifite to return to the boring machines of the marquis Montalembert, those of M. de Maritz being only good for brafs or very foft iron cannon. M. Montalembert, is one who understands this art of cannon founding the best, and I have always lamented that his zeal for all the necessary knowledge of this kind, has only tended to the ruin of his fortune. As I live far diffant from him, I wrote this memoir without communicating it to him; but I shall be more flattered with his approbation than that of any other person, for I do not know any one who better understands what is here mentioned. If we were to collect in one mass in the kingdom of France, the enlightened treasures which are thrown afide or disdained, we should foon be the most flourithing and richest nation in the For example, he was the first who advised the discovery of the resistance of cast iron by its specifical weight; he also endeavoured to perfectionate the art of moulding cannons in fand, and this art is loft fince they have began to turn them. With moulds of earth, earth, formerly used, the surface of cannon was always charged with asperities and roughnesses. M. de Montalembert best found that mode of moulding in sand, which gave a fine polish to the surface. Those who understand the arts, will perceive the difficulties he had to surmount to attain this end, and the pains required to be taken to form workmen capable of executing these moulds, to which the bad custom of turning having been substituted. An excellent art has been

loft to adopt a fatal practice.

A very necessary attention when a cannon is cast is to prevent the drofs which fwims on the metal, from falling with it into the mould. The lighter the cast, the greater will be the drofs, and we may judge by the inspection of the running, whether the cast be of a good quality or not, for then the furface is smooth and has no drofs. But in all thefe cafes, we must take care to compress the running matter by many straw whisps placed in the gutters; with this precaution, but little drofs passes into the moulds, and if the cast is dense and compact, there will be none at all. The waste of the cast commonly proceeds only from its being too crude and too precipitately melted. Befides, the heaviest matter comes first from the furnace: the breech of the cannon is always for this reason of a better matter than the upper part of the piece; but there never is any waste in the cannon, if, on one hand the drofs is stopped by straw, and that at the same time we give it a strong mass of superabundant matter, of which it is even as necessary as useful, that there still remains, after the running, three or four hundred weight inusion in the crucible. This metal which remains supports the heat, preserves the bottom of the furnace and prevents the melting ore from burning.

It seems, that in France, cannon have been often cast with rock ore, which contains a greater or lesser

quan-

quantity of sulphur; and as it is not customary to burn it in our provinces where wood is dear, as is practised in the northern countries where wood is plentiful; I presume, that the brittle quality of our cannon might also proceed from this sulphur, which is not separated from the ore before it is thrown into the fusing surnace. The founderies of Rouelle, St. Gervase, and Baigorry, are the only ones I am acquainted with, and that of Nouee, which I have spoken of. In all these four, I believe, only rock ore is made use of; and I have not heard that they only roast it at St. Gervasse and Baigorry: I have strove to procure angles of those ores. The following is what M. de Morogues has written to me on the subject of the ores used at Rouelle.

"The first is hard, compact, heavy, and strikes fire with steel: it is of a brown colour, and formed of two coats of unequal thickness, one of which is spongy, and sprinkled with holes or cavities, of a violet colour, and some of an indigo hue, with small protuberances, bordering on a sanguine colour: characters which might rank it in the ninth class of the art of forges, as a kind of hematite stone, if it was not rich and kind.

"The second resembles the preceding, with respect to weight, hardness, and colour; but it is
a little falardee (We term Salard, or Salarded Ore,
that which has clear dark grain, mixed with a
grey sand of slint and iron). It is rich in metal,
used with very kind ore it melts-readily: its coat
when broken is streaky, and sprinkled often with
cavities of a brownish colour. It appears to be of
the fixth kind of red ore in the art of forges.

"The third, named in the country Glassy, because it commonly has a smooth appearance, and
foft to the touch, is neither very heavy, nor very
rich: it has commonly some small black and
glassy points, of a grain resembling Morocco leather.

"ther. Its colour is varied, of a lively red, brown.

or yellow, and a little green with fome cavities.

" It appears, by reason of its close and glassy sur-

faces, to have some affinity with the specular ore

of the eighth class.

"The fourth, which affords excellent iron, but in a small quantity, is light, spongy, soft, and of a dark brown colour, having some little fandy eminences. It appears to be a fort of clay ore

of the eleventh kind.

"The fifth is a falarded ore, affording much fire with steel: it is hard, compact, heavy, and se sprinkled with small brilliant specks, which are only fand of the colour of wine lees. This ore is " difficult to fuse; the quality of its iron is said on not to be bad, but it produces but little: the " workmen pretend, that there is no method of " melting it by itself, and that the abundance of fparks which separate from it, agglutinates in the " furnace: this ore does not appear to have any " well characterized refemblance, with that which

" Swedemborg speaks of."

"A great number of other kinds of ore is made " use of; but they differ from the preceding only by less quality, excepting a kind of iron ore which may furnish a fixth class. This ore is plen-" tiful in mines, and eafily extracted; it is yellow, " and fometimes mixed with small grains; it affords " little iron, and may be ranged in the twelfth " kind.

"The coats of all the ores of this country, is a vitrifiable earth, fcarcely clayey. All thefe " ores are mixed, and the earth extracted from

" them is almost fandy.

"They term Schiffre in Augoumois, a flint like " fire-stone, and which affords much fire when struck " against steel: it is of a clear yellow, very hard, et and fometimes partakes of matters which may

" have iron, but it is not schift.

"Castina is a true calcareous stone, tolerably pure, if we may judge by the uniformity of its breaking, and its colour, which is grey: it is heavy, hard, and takes a very smooth polish."

By this recital of M. de Morogues, it feems, that there is only the fixth kind which does not require to be roafted, but only well worked before it is

thrown into the furnace.

On the whole, although generally speaking, and as I have observed, rock ore which is found in large folid maffes, owe their origin to the element of fire; nevertheless, there is found also many iron ores in pretty large masses, which are formed by the motion and intermedium of water. By the loadstone we may diffinguish those which have undergone the action of fire, for those will always be magnetical; whereas, those which have been produced by the stillation of waters, are not so at all, and will not become fo, till after having been roafted and almost liquified. These rock ores which are not attractable by the loadstone, contain no more fulphur than our ore in grain; therefore, the operation of roafting them, which is very expensive, must then be suppressed.

I have endeavoured to present in this Memoir, all what I have thought might be useful for the melioration of our sea cannon. I perceive, at the same time, that many things remain to be done, especially to procure in each soundery a pure and compact cast, to have a resistance superior to all explosion; nevertheless, I do not think that it is at all impossible; and I imagine, that by purifying the iron as much as possible, we should attain to that point, that the piece would only split, instead of slying by too strong a charge; if I could once obtain this end,

there

there would no longer remain any thing to fear of defire in this respect.

EXPERIMENTS on the Strength of WOOD.

THE principal use of wood in buildings and ftructures of all kinds, is as a support: the practice of workmen who use it, is only founded on trials, often, in fact, reiterated, but always rude. They but very imperfectly know the strength and refistance of the materials they work with. I have endeavoured to determine, with fome precision, the strength of wood, and the means to render my labours useful to builders and carpenters. To attain this end, I have been obliged to break many beams and many timbers of different lengths. We shall find, in the course of this Memoir, the exact detail of all these experiments; but I shall first present their general refults, after having faid a word or two on the organization of wood, and of some particular circumstances which appeared to me to have escaped persons who are employed in these matters.

A fire is an organized body, whose structure is not yet well known. The experiments of Grew, or Malpighi, and especially those of Hales, have, in sact, afforded great lights on the vegetable economy; and, it must be owned, that we are indebted to them for almost all we know of this matter; but in this, as well as in many others, we are ignorant of more than we know. I shall not here make an anatomical description of the different parts of a tree, as it would be useless for my design; it will be sufficient to give an idea of the manner in which trees grow, and the manner in which wood is formed.

A feed of a tree or a nut thrown in the earth in the spring, at the expiration of a few weeks produces a small, tender, and herbaceous stalk, which

increases, extends, thickens, hardens, and already contains, at the end of the first year, a thread of lignous substances. At the extremity of this young tree, is a bud which expands the following year, and from whence a stalk issues similar to that of the first year, but stronger: this thickens and extends still more, hardens at the same time, and produces another bud, containing the shoot of the third year, and so on, till the tree has attained its height. Each of those buds is a kind of germ, which contains the tree of each year. The growth of trees in height is made, therefore, by many fimilar and annual productions; so that a tree 100 feet high, is compased in its length of many young trees, the longest of which is often only two feet high. All these young trees, never alter their dimensions; they exist in a tree of an hundred years without thickening or enlarging, and only become more folid. Thus, then, is the growth in height made; and the growth in bulk depends thereon. This bud which formed the fummit of the young tree of the first year, derives its nutriment from the substance, and even the body of this young tree: but the principal canals, which serve to conduct the sap, are found betwixt the bark and the lignous part. The action of this fap in motion, dilates these canals and enlarges them; while the bud is raifing, it draws and lengthens them: besides, and the sap continually flowing, deposits those fixed parts which augments the solidity. Thus, after the fecond year, the tree already contains a lignous thread in form of a very long cone, which is the first production of the wood of the first year, and a lignous coat as conical, which furrounds this first thread, and furmounts it, and which is the foundation of that of the fecond year. The third coat is formed like the fecond, and it is the same with all the rest, which successively and continually surround it; fo that a large tree is a composition of a great Vol. V. Ff number VOL. V.

number of lignous cones, which furround and cover each other as long as the tree grows. When it is felled, the number of these cones are easily reckoned on the transversal stroke of the trunk, whose fections form circles, or rather concentrical crowns: and we discover the age of the tree by the number of these crowns; for they are distinctively separated from one another. In a strong oak the thickness of each coat, or crown, is about two or three lines: this thickness is hard and folid wood; but the substance which unites these crowns together, the prolongation of which forms lignous cones, is not near fo firm; it is the weakest part of the wood, whose organization is different from that of the cones, and depends on the mode in which these cones fix and unite to each other, and which we are going to explain in a few words. The longitudinal canals which carry the nutriment to the bud, not only takes extent and acquires folidity by the action and deposit of the pith, but they also strive to extend in another manner; they branch out, and fend forth little filaments like fmall branches, which on one fide produce the bark, and on the other fix themfelves to the wood of the preceding year, and form between the two coats of the wood a spongy network, which, but transversally even to a very great thickness, shews many small holes. The coats of wood are, therefore, united to one another by a kind of network, which does not near occupy fo much room as the lignous coat: it is not above half a line thick; and this thickness is nearly the same in all trees of the like kind; whereas, the lignous coat is more or less thick, and is confiderably varied in the same kind of tree, as in the oak, that I measured, some of which were three lines and an half, and others only half a line thick.

By this fimple exposition of the texture of wood, we perceive that the longitudinal coherence must be much

much more confiderable than the transversal union. We perceive, that in small pieces of wood, as in a piece of an inch thick, if fourteen or fifteen lignous coats are to be met with, there will be thirteen or fourteen partitions; and, confequently, this piece of wood will not be fo strong as a like piece, which shall contain only five or fix, and four or five partitions. We also see, that in these small pieces, if one or two lignous coats are met with, which are cut by the faw, which often happens, their strength will be confiderably diminished; but the greatest defects of these small pieces of wood, which are the only ones on which experiments have hitherto been made, is, that they are not composed like large pieces. The position of the lignous coats and partitions in a bar of wood, is very different from the position of these coats in a beam; their figure is even different, and, confequently, we cannot effimate the strength of a large piece by that of a bar. A moment's reflection will make what I have faid, quite evident. To form a beam, it is only necessary to square the tree; that is to say, take off four cylindrical fegments of a white and imperfect, or the fappy part of the wood. In the heart of the tree, the first lignous coat rests in the middle of the piece, all the rest surround the first in form of circles, or cylindrical crowns: the greatest of these circles has a diameter as thick as the piece; beyond this circle, the rest are cut, and form only portions of circles which diminish towards the edge of the piece. Thus a fquare beam is composed of a continued cylinder of good folid wood, and of four angular portions of a less folid and younger wood. A bar cut from the body of a large tree, or taken in a plank, is quite otherways composed. There are small longitudinal fegments, which are sometimes placed parallel to one of the furfaces of the bar, and at others are more or

less inclined, from the segments which are longer or thorrer, and, consequently, stronger or weaker ; besides, there is always two positions in a bar, one of which is more advantageous than the other. If you place the bar so that these planes are vertical, it will refift much more than if in an horizontal pofition: that is, as if we broke many planks at one time, they would refift much more being placed on the fide than on the flat. These remarks renders it already perceptible, how little we must rely on the calculated tables, on the formulas fet down by different authors, on the strength of wood, which they have tried only on pieces, the thickest of which was not above one or two inches, and of which they give us neither the number of the lignous coats which those bars contained, nor the portion of them, nor the direction in which they were found when they broke these bars: circumstances, nevertheless, very effential, as we shall perceive by my experiments, and by the care which I took to discover the effects of all these differences. Physicians, who have made experiments on the strength of wood, have paid no attention to these inconveniencies; but there are others, perhaps, still greater, which they have also neglected to forefce. Young wood is not fo strong as old. A bar taken from the foot of a tree refifts more than one from the top; a bar taken at the circumference, near the fap, is not fo ftrong as a like piece taken at the center of the root: in other respects, the degree of dryness of wood adds much to its refistance. In short, the time employed in charging the pieces to break them, must also be put in confideration, because a piece which will fustain a certain weight during some minutes, cannot sustain this weight during an hour; and I have found, that timbers which had each supported 9000 weight a whole day without breaking, broke in five or fix months months under the load of 6000 weight; that is to fay, that they did not bear for fix months two thirds of the weight they had bore during one day. All this proves sufficiently how imperfect the experiments which have been made on this matter are; and, perhaps, this also proves, that it is not very

eafy to perform them well.

My first trials, which are in a great number, have only served to point out the inconveniencies I speak of. I at first broke some bars, and calculated what must be the strength of a longer and thicker bar, than those which I had tried, and having compared the result of my calculation with the actual load, I found such great difference, that I repeated the same thing a number of times without coming near the calculation of the experiments: I tried on other lengths and thicknesses, the event was the same: at length, I determined to make a complete course of experiments which might serve me to form a table of the strength of wood, on which I might rely, and all the world consult when they had occasion.

I shall relate in as few words as possible, the method in which I executed my project. I chose one hundred found oaks out of my woods, as close to each other as we could find them, in order to have the wood from the same soll, for trees of different countries and different foils have different relistances. Another inconvenience which alone feemed to fet afide all the utility I expected to derive from my la-These oaks were of the same handsome kind. and produced an acorn or two on the branch: the fmallest of these trees was two feet and an half in circumference, and the largest five. I chose them of different fizes, in order to come the nearer common use. When carpenters have need of a piece five or fix inches square, they do not cut it out of a tree which may allow a foot; the expence would

be too great; and it but too often happens, that they use too slender trees, and where they leave much sap; for I do not here speak of sawed timbers sometimes made use of, and which is cut from a larger tree; nevertheless, it is just observe, en passant, that these trees are weak, and their use ought to be proferibed.

As the dryness of wood raises its resistance considerably, and as, besides, it is very dissipute to ascertain this degree of dryness, since often in two trees felled at the same time, one will dry sooner than the other. I was desirous of avoiding this inconvenience, which might have deranged the course of my experiments; and I thought I should have had a more fixed and certain term by taking greenwood: I therefore felled the trees one by one as I wanted them. The same day a tree was felled, it was carried to the place where it was to be broken: I had the exact dimensions given to it, and in the

morning it was tried.

The machine with which I made the greatest number of experiments, was constructed as follows. Two strong timbers feven inches square, three feet high, and as long, strengthened through their middle by an upright: on these timbers the ends of the pieces defigned to be broken was placed. Many square clamps of round iron, the thickest of which was near nine inches in its internal breadth; the fecond feven, and the rest smaller. The piece that was to be broken was put into the iron ring; the largest ferved for the thickest pieces, and the smallest for bars; each had on the infide a ridge, made to hinder the ring from inclining, and also to shew the breadth of the iron on the wood which was to be broken. In the inferior part of this fquare iron, two were forged of the same fize as the iron of the ring these two divided, and formed a round about nine inches diameter, in which a wooden peg was put of the same size, and sour feet long. This key bore a strong table, fourteen seet long by six broad, made of timbers sive inches thick, placed one against the other, and held there by strong bars. This we suspended by means of the large wooden peg, and it served to put the weights in, which consisted of 300 pieces of stone, which weighed each 28, 50, 100, 150, and 200lb. These stones were put upon the table, and a mass of broad and long stones was piled up, and as high as was necessary to break the piece.

Eight men continually kept loading the table, and placed on the center 200 weight, then 150, 100, 50, and so on down to 25. Two men on a scaffold placed the weight of 50 and 25lb. which could not be put on from the ground without running a risk. Four other men applied and supported the four corners of the table, to hinder it from swinging, and to keep it in equilibrium: another with a long wooden rule, observed how much the piece bent in proportion as it was loaded, and another set down the time, and wrote the weight, which often

amounted to 20, 25, and 28000lb.

In this manner I broke many hundred pieces of wood, as well beams as other flighter pieces without reckoning three hundred bars, and this great number of laborious trials has been scarcely sufficient to afford me a scale of the strength of wood, for all sizes and lengths. I formed a table, which is at the end of this Memoir: if it is compared with those of Muschenbroek, and other physicians who have employed themselves on this subject, it will be perceived how greatly their results differ from mine.

In order to give a just idea of this operation, by which I broke the pieces of wood to discover the strength of them, I shall relate the exact process of

one of my experiments, by which we may judge of all the rest.

Having fell'd an oak five foot in circumference, I had it brought and worked the fame day by carpenters. The next morning the workmen reduced it to eight inches square, and twelve inches long. Having examined this piece with care, I judged that it was very good; it had no other defect than a small knot. The next morning I weighed this piece, and found it to weigh 409lb. Afterwards having put it into the iron ring, and having turned that fide upwards on which the knot was, I put the piece on the timbers. Having afterwards flided the iron ring to the middle of the piece, we lifted up by levers the table, which alone with the rings and key weighed 2500lb. At fifty-fix minutes after three, eight men began to load the table, at thirty-five minutes after five the piece had only bent two inches, although loaded with 16000lb. In fix minutes more it bent to two and a half, being then loaded with 1850olb. In fix minutes more it bent to three inches, and was loaded with 21000lb. At one minute after fix it bent to three inches and a half and was loaded with 23625lb. when it made report like a pistol, and immediately discontinued from loading it, and the piece bent half an inch more, i. e. twenty-four inches in the whole. It continued to make great reports for an hour, and out of the ends iffued a kind of fmoke with a hiffing noise. It bent near seven inches before it absolutely broke, and during this time it supported the weight of 23625lb. A part of the lignous fibres were cut asif they had been fawed, and the remainder broken in shivers, leaving vacancies like the teeth of a comb, the edge of the ring which was about three times broad and which bore all the load, was entered about a line and an half into the wood, and on each fide a bundle of fibres, and the little knot which was

the

on the upper fide, had not at all contributed to its

breaking.

I have a journal of above one hundred experiments of the fame kind, which I made on pieces of different fizes, to be affured of their respective strength. The first remark I made, is, that wood never breaks without giving notice, at least, if the piece is not very fmall or very dry. Green wood breaks more difficultly than dry, and in general wood which has a fpring, refifts much more than that which has not. The fap, the branches, the tops, and all young wood is weaker than old: the strength of the wood is not proportionable to its volume, a double or quadruple piece of the fame length, is much stronger in proportion than another. It does not require 4000 lb. to break a piece ten foot long, by four inches square, and it requires ten to break a double piece, it requires 26000 lb. to break a quadruple piece, i. e. a piece ten foot long by eight inches square. It is the same with respect to length; a piece of eight feet and of the same thickness as a piece of fixteen feet, must by mechanical laws be just double, nevertheless it bears much less. I could give physical reasons for all these circumstances; the wood which in the same soil grows the fastest, is the strongest: that which has grown the flowest and whose annual circles, i. e. the lignous coats are thin, is weaker than the other.

I found that the strength of wood is proportionable to its weight, insomuch that a piece of the same length and size, but heavier than another piece, will be likewise stronger nearly in the same ratio. This remark affords means of comparing the strength of wood of different countries and soils, and infinitely extends the utility of my experiments; for, when we are employed on an importan, structure, or a work of consequence, we shall easily be able, by means of my table, and by weighing the pieces, or parts of them, to ascertain the strength of the wood used, and avoid

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the double inconvenience of using too much or too little of this matter, which is often too prodigally misapplied and sometimes managed with still less

judgment.

It might be imagined, that a piece, which, as in my experiments is weighed on two treftles, ought to bear much less than a piece fastened by the two ends and fixed in a wall, as are the beams and girders of buildings: but, if it is confidered that a piece, which I suppose twenty-four foot long, bending fix inches in its middle, which is often more than is required to break it, raises at the same time only half an inch at each end, because the load draws the end out of the wall, often much more than it raifes it. We shall perceive that my experiments are applied to the common pofition of beams in a building: the power which causes them to break, by forcing them to bend in the middle and to raise them at the ends, is an hundred times more confiderable than that of the plaister and mortar which eafily gives way, and I can affert, from proof, that the difference of the strength of a piece placed on two fupporters and free at the ends, and of that of a piece fixed by the two ends into a wall, is fo trifling, as not to deferve attention.

I own, that by holding a piece by iron anchors, placed on free-stone in a good wall, confiderably increafes its strength. I have some experiments on this position, of which I can give the refults: I shall befides acknowledge, that if the piece was invincibly retained by the two ends in an inflexible and perfeetly hard matter, it would require an almost infinite power to break it; for we can demonstrate that, to break a piece in fuch a fituation, would require a much greater than the necessary power to break a piece of wood upright, which is drawn or pressed according to its length.

o afcerrain the firength of the wood wled, and avoid

In buildings the wood is loaded its whole length and in different points; whereas in my experiments, the whole load is united in one fingle point at the middle. This makes a confiderable difference, but it is eafy to determine it exactly in a calculation that all builders who are a little veried in mechanism can eafily supply.

To compare the effects of time on the reliftance of wood, and to discover how much its strength diminishes, I chose four pieces, eighteen foot long by feven inches thick; I broke two which bore good lb. each during one hour; I had the other loaded with 6000 only, and left them thus loaded, refolved to wait the event. One of these pieces broke at the expiration of five months and twenty-five days, and the other at fix months and feventeen days. After this experiment, I got two other pieces perfectly alike, and only loaded them 4500 lb.. I kept them thus loaded for two years, they did not break, but only bent very confiderably. Therefore, in structures for long duration, we must only give half the load to wood which may breakit, and it is only in fuch preffing occasions, and in structures which should not remain, as in a temporary bridge to pass an army, or a fcaffold to befiege or affail a town, that we may hazard two thirds of the weight the wood can bear.

I do not know, whether it is necessary to observe here, that I rejected several pieces which had defects, and that I have not comprehended in my table only the experiments of which I have been satisfied. I also refused more wood than I used; knotty crossgrained and other desective wood is easily perceived, but it is difficult to judge of their essect by relation to the strength of a piece, it is certain, that they greatly diminish; and I have sound a mode of essection mating nearly the diminution of strength caused by a knot. It is known, that a knot is a kind of adherent peg to the internal part of the wood; we can even know, by the number of annual circles which

it contains, and the depth to which it penetrates. I have made holes in form of cones, and of the fame depth in pieces which were not knotty, and filled them with pegs of the same figure. These pieces I broke, and discovered, by that how much strength knots deprive wood of, which is much beyond what might be imagined. A knot, or a peg in the under fide, and especially in one of the edges, sometimes diminishes the strength of the piece one fourth. I have also attempted to discover by many experiments the diminution of strength caused by the cross-grain of the wood. I am obliged to suppress the results of those proofs which take up too much room: nevertheless, it is permitted me to relate a circumstance which will appear remarkable; which is, that having broke fome crooked pieces, fuch as are used for ships, domes, &c. I found that they refifted more by oppofing the weight to the concave fides; though it might be thought, that by opposing the convex side, as the piece for the vault, it would refift the most. This would be the cafe with respect to a piece, whose longitudinal fibres should be naturally crooked, i. e. a crooked piece whose grain is continued and not crossed; but as the crooked pieces I made use of, and almost all those used in structures, are taken from thick trees, the internal part of these coats is much more croffed than the external, and confequently refift the less, as I have found by experience.

It should seem that experiments made with so much care and in such great numbers should leave nothing wanting, especially in a matter so simple as this; nevertheless I must agree, and will freely own, that many things still remain to be sound out; I shall only quote some. We do not know the relation of the longitudinal coherence of wood to the power of its transversal union; what sorce is necessary to break, and what to split a piece of wood. We do not know the resistances of wood from positions different from

that

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hat which my experiments support; positions nevertheless common enough in buildings, and on which it would be very important to have certain rules. I speak of the strength of upright wood, wood inclined, and wood held by one of its extremities, &c. But by observing from the results of my labour, we shall be able to attain easily to this knowledge which we are now desicient in. Let us go on with the detail of my experiments.

I at first sought after the density and weight of oak in its different ages, and what proportion there was between the weight of the wood which occupied the centre, that of the circumference, and also between the weight of perfect wood and that of the sap, &c. M. Duhamel told me, he made some experiments on this subject. The scrupulous attention, with which mine have been made, gives me room to think, that

they will agree with his.

I had a block of wood brought to me, taken from the foot of an oak felled the fame day, and having fixed the point of a compass to the center of the annual circles, I described a circumference around this center, and afterwards having placed the point of the compass to the middle of the thickness of the sappy part, I described a like circle; I afterwards took fmall cylinders from this block, one from the heart of the oak and the other from the fap, and having weighed them hydroftatically and found each weighed 371 grains; having afterwards weighed them feperately in water I only plunged them in a moment, I found the piece of the heart to weigh 317 grains, and the other 344. The short time they remained in the water, reduced the difference of their increase in volume by the imbibition of the water, which is quite different in the heart of the oak and fap.

The fame day I made two other cylinders, one of the heart and the other of the fap of the oak, taken out of a tree of nearly the fame age as the first. These two cylinders weighed each 1978 grains, the piece of the heart of oak lost 1635 grains in water, and the other piece 1784. By comparing this experiment with the first, we found the heart of oak lost in this second experiment only 307, or about 371, instead of 317½, and so likewise the sappy part lost 330 of 371 grains, instead of 344, which is nearly the same proportion between the heart and sap. The real difference proceeds only from the different density as well of the heart as of the sap of the second tree, the whole wood of which in general was more solid

and harder than the wood of the first.

Three days after, I took three cylinders from another piece of oak felled the fame day as the preceding, one from the centre of the tree, the other from the circumference of the heart, and the third at the top; all these weighed 975 grains in air, and having weighed them in water, the wood of the center loft 873 grains, that of the circumference of the heart 906, and that of the fap 938. By comparing this third experiment with the two preceding, we found that 371 grains of the heart of the first oak, weighing 317 grains and a half, 371 grains of the fecond oak ought to have loft nearly 332 grains, fo likewife 371 grains of the fap of the first oak losing 344 grains, 371 grains of the fecond should have lost 330 grains, and 371 grains of the fap of the third cak ought to have loft 356 grains, which is not far distant from the first proposition; the real difference of the loss, as well of the heart as of the fap of this third oak, proceeding from its wood being lighter and dryer than that of the two others. Taking therefore the mediate measure between these three different pieces of oak, we find that 371 grains of heart, lofes 319 grains, one third of their weight in value, and that 371 grains of fap loses 343 grains of their weight. Therefore the volume of the heart of oak is to that of the fap :: 3191: 343 and the maffes :: 343: 3791 which makes about a fifteenth difference between the specific

weight of the heart and fap.

For the third experiment I chose a piece of wood whofe lignous coats appears pretty equal throughout their thickness, and I took off feven cylinders, in fuch a manner that the center of my middle cylinder, which was taken at the circumference, of the heart, was equally remote from the center of the tree from whence I had taken my first cylinder of the heart, and from the center of the cylinder of the fap: by that I discovered that the weight of the wood decreases merely in an arithmetical proportion for the lofs of the central cylinder, being 873, and that of the fappy cylinder being 938, we shall find by taking of the half of the fum of these two numbers, that the wood of the circumference of the heart must lose 9091, and by experience I find that it lost 906; therefore the wood from the center to the last circumference of the sap, diminishes in density in an arithmetical proportion.

By trials like those I have indicated, I am certain, of the diminution of the weight of the wood in its length: the wood of the foot of a tree weighed more than the wood of the trunk about the middle of its height, and that of this middle part weighed more than the wood of the top, and that nearly in an arithmetical progression, as long as the tree continued growing. But there is a time when the central wood, and that of the circumference of the heart weighs nearly equal, and this in the time when the wood is

in its perfection.

These experiments were made on trees fixty years old, which still were growing, and having repeated them on trees of forty-fix years and on trees of thirtythree years, I have always found that the wood from the center to the circumference, and from the foot ofthe tree to the top, diminishes in weight nearly in an arithmetical progression.

But, as I just observed, as soon as the trees ccase from growing, this proportion varies. I took three cylinders out of the trunk of a tree about an hundred years old, which weighed 2004 grains in air: that of the center lost 1713 grains in water; that of the circumference of the heart, 1718 grains, and that

of the fap, 1779 grains.

By a second trial, I found, that of three cylinders, taken from the trunk of a tree about 110 years old, and which weighed 1122 grains; that of the center, lost 1602 grains in water; that of the circumference of the heart, 997 grains; and that of the sap, 1023 grains. This experiment proves, that the heart was not the most solid part of the tree; and it proves, at the same time, that the sap is heavier and more solid in old than in young trees.

I own, that in different climates and foils, it varies prodigiously, and that trees may be found fortunately enough fituated to grow at the age of 150 years: these form an exception to the rule; but, in general, it is constant, that wood increases in weight to a certain age in the proportion we have established, and that after this age, the wood of different parts of the tree becomes nearly of an equal weight, and that it is then in its perfection; and, at length, on its decline, the center of the tree stopping growth, the wood of the heart dries in defect of sufficient nutriment, and becomes lighter than the wood of the circumference, in proportion of the depth, difference of foil, and number of circumstances which may prolong or shorten the time of the trees growth.

Having by the preceding experiments discovered the different density of wood in the different ages and states it is in before it arrives to perfection, I sought after the difference of its strength also in the like different ages; and, for that purpose, I took

from

from the centers of feveral trees, all of the fame age; that is, about fixty years, many bars three feet long by an inch square, among which I chose four that were most perfect; they weighed,

1ft. 2d. 3d. 4th Bar.
ounces. ounces. ounces.
$$26\frac{3^{1}}{3^{2}}....26\frac{18}{3^{2}}....26\frac{16}{3^{2}}....26\frac{15}{3^{2}}$$

They broke under the weight of 301, 289, 272,

272 lb.

I afterwards took feveral pieces of the circumference of the heart, of the same length and square, i.e. of three feet by one inch, amongst which I chose four of the most perfect, which weighed,

They broke under the weight of 262, 258, 255, 253lb.

And so likewise, having taken four pieces of the sappy part, they weighed,

Ift. 2d. 3d. 4th. ounces. ounces. ounces.
$$25\frac{5}{32}$$
... $24\frac{2^1}{3^2}$... $24\frac{2^6}{4^2}$... $24\frac{2^4}{3^2}$

They broke under the weight of 248, 242, 241,

250lb.

These trials made me suspect, that the strength of the wood might possibly be proportioned to its weight, which I found true, as will be seen in the course of this Memoir. I repeated the like experiments on two-seet bars, on others eighteen inches Vol. V.

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long by one inch fquare. The refult of these experiments is as follows:

BARS of Two Feet.*

	got	Weight		
	ıft.	2d.	3d.	4th.
	ounces.	ounces.	ounces.	ounces.
Center.	17=	$16\frac{3^1}{3^2}$	$16\frac{24}{92}$	$16\frac{24}{33}$.
Circumf.		$15\frac{1}{32}$		
Sap				14 = 22.
Circumf.	.356	428 ¹ 350	346	346.
	BAR	S of Eig	gbteen In	ches.
n -		Weigh	t.	
250, 25	ounces.	2d.	3d.	4th.
Canton	7.010		- 4	T.A.

0	Weight.				
154, 25	ıft.	2d.	3d.	4th.	
	ounces.	ounces.	ounces.	ounces.	
Center	13 =	1 3 6	13 4	13	
Circumf.	1216	12 =	12 =	124.	
	0-	0-	1 1	0-	
	. 32	32	32	32	

^{*} It must be remarked, that as the tree was pretty large, the wood of the circumference was much more distant from that of the center than the sappy part.

BARS of One Foot.

		Weigh	t.	
e a sur la res	ıft.	2d.	3d.	4th.
	ounces.	ounces.	ounces.	ounces.
Center .	819	819	816	$8\frac{15}{32}$.
	32	32	32	32
Circumf	81	$7\frac{32}{32}$	7-0	720
	32	32	32	32
San	-10	7_2		628
Dap	. , /30	7 =		32

Loaded with. Center. 764¹.....761¹.....750¹.....751¹. Circumf. 722......700.....693.....698. Sap...668.....652......643.

By comparing all these experiments, we see that the strength of wood does not very exactly follow the same proportion as its weight; but we always find, that this weight diminishes, as in the first experiments, from the center to the circumference. We must not be astonished, that these experiments are not sufficient to judge exactly of the strength of wood: for bars, taken from the center of the tree, are otherwise composed than the bars of the circumference, or of the sap; and I was not long before I perceived, that this difference in the position, as well of the lignous coats, as of the partitions which unite them, must have great influence on the resistance of wood.

I therefore more attentively examined the form and situation of the lignous coats in the different bars taken from the different parts of the trunk of the tree: I perceived, that the bars taken from the center contained a cylinder of round wood in the middle, and were only crooked at the edges: I perceived, that those of the circumference of the coat formed

formed planes between them almost parallel with a very sensible curvature, and that those of the sap were almost entirely parallel with no sensible curvature. I observed, besides, that the number of the lignous coats varied very considerably in different bars, so that there were some which contained sourteen in the thickness of an inch. I perceived also, that the position of these lignous coats, and the direction in which they are found when the bar is broken, must still vary their resistance; and I searched after the means of justly knowing the proportion of this variation.

I cut from the foot of the fame tree, at the circumference of the heart, two bars three feet long by an inch and a half fquare; each of them contained fourteen lignous coats almost parallel. The first weighed 3lb. 2 ounces \(\frac{1}{2} \); and the second, 3lb, 2\(\frac{1}{2} \) ounces. I broke these two bars, by exposing them in such a manner, that in the first the lignous coats were found placed horizontally, and in the second, vertically: I foresaw, that this latter position must be advantageous; and, in fact, the first broke under the load of 832lb. and the second broke only under that of 972lb.

I likewise took many small bars of an inch square by a foot long: one of these bars, which weighed 7 ounces 30, and contained twelve lignous coats placed horizontally, broke under 784lb. The other, which weighed 8 ounces, and contained also twelve lignous coats, placed vertically, broke only

under 860lb.

Of two other similar bars, the first of which weighed 7 ounces, and contained eight lignous coats; and the second, 7 ounces $\frac{10}{3}$, and contained also eight lignous coats. The first, whose lignous coats were placed horizontally, broke under 778lb.

and the other, whose coats were placed vertically, broke under 828lb.

I likewise cut bars two feet long by one and an half square: one of which that weighed 2lb. 7 ounces 1.16th, and contained twelve lignous coats, placed horizontally, broke under 1217lb. and the other, which weighed 2lb. 7 \frac{1}{3} ounces, and which contained also twelve lignous coats, broke under

1294lb.

All these experiments concur to prove, that a bar or a girder resists much more when the lignous coats which compose it are situate perpendicularly: they prove also, that the more lignous coats there are in the bars or other small pieces of wood, the more the difference of those pieces in two opposite positions is considerable. But, as I was not yet fully satisfied in this respect, I made the like experiment on planks placed one against the other, and I shall relate them in the course of the work, not chusing here to interrupt the order of the time of my labours, because it is more natural to give things as

they were done.

The preceding experiments have served me for a guide to those which follow: they learnt me, that there was a great difference between the weight and strength of the wood of the same tree, according as the wood is taken at the center or circumference of the tree: they pointed out to me, that the situation of the lignous coats, causes the resistance of the same wood to vary: they taught me, that the number of lignous coats have great influence on the strength of wood; and from hence I discovered, that the attempts which have hitherto been made, are insufficient to determine the strength of wood; for they have all been made on small pieces of an inch or an inch and an half square; and on these experiments the calculation for the table we have given

for the refistance of beams, rafters, and pieces of all

fizes and lengths have been made.

After this primary knowledge of the strength of wood, which are only incomplete notions, I endeavoured to acquire more precise: I was desirous of ascertaining whether two pieces of wood of the same length and shape, but the first of which was double the thickness of the second, the first was double the resistance: for this reason I took several pieces from the same tree, and at the same distance from the center, having the same number of years, and situated in the same manner, with all the necessary circumstances to establish a just comparison.

At the same distance from the center of the tree. I took four pieces of found wood, each two inches fquare by eighteen inches long: these broke under 3226, 3062, 2983, and 2890lb, i.e. under the mediate load of 3040lb. I likewise took four pieces of feventeen lines, which made nearly half the fize of the four first pieces of an inch square by the same length of eighteen inches, which makes the quarter of the fize of the first, and I found that they broke under 526, 517, 500, and 496lb. i.e. under 510lb. This experiment evinces, that the strength of a piece is not proportional to its fize; for these fizes being 1, 2, 4, the loads ought to have been 510, 1020, 2040; whereas, they are, in fact, 510, 1252, 3040, which is very different, as has been already remarked by fome authors who have written on the refistance of folids.

I took likewise several bars of a foot, eighteen inches, two feet, and three feet long, to discover whether the bars of a foot would bear as much again as those of two feet, and to be as certain whether the resistance of the pieces diminishes exactly in the same ratio as their length increases. Bars of a foot will support 765lb. those of eighteen inches, 500lb. those of two feet, 369lb. and those of three feet, 230lb.

230lb. This experiment left me in doubt, because the weights were not very different from what they ought to be; for, instead of 765, 502, 369, and 230lb. the rule of a lever requires 765, 510½, 382, and 255lb. which is not distant enough to conclude, that the resistance of the pieces of wood does not diminish in the same ratio as their length increases; but, on the other hand, it is far enough removed for us to suspend our judgment; and, in sact, we shall perceive, in the course of the work, that we have here reason to doubt of it.

I afterwards tried what was the strength of wood, supposing the piece unequal in its dimensions; for example, by supposing it an inch thick by an inch and an half broad, and by placing it on the one and afterwards on the other of these dimensions: for this purpose, I made four bars of the sap, eighteen inches long by an inch and a half on one facies, and an inch on the other: these four, placed on an inch facies, supported 723lb. and four other perfectly fimilar bars, placed on an inch and a half facies, supported 93511b. four bars of found wood, placed on an inch facies, supported 775lb. and on an inch and an half, 9981b. It must always be remembered, that in these experiments, I took care to chuse pieces of wood nearly of the same weight, and which contained the same number of lignous coats placed in the same direction.

With all these precautions I could scarcely give myself satisfaction. I often perceived irregularities and variations which de-ranged the consequences I wanted from my experiments, and I have above a thousand accounts of designs I made, which, nevertheless, I could not derive any thing from, and which lest me in an uncertainty in many respects. As all these experiments were made with pieces of wood of an inch to two inches square, a very scrupulous attention was necessary in the choice of the

wood; an almost perfect equality in the weight, and the same number of lignous coats; besides which. there was an almost inevitable inconvenience from the obliquity of the fibres, which often rendered fome of the pieces of a whole coat, and others of half a one, the which confiderably diminished the strength of the bar. I speak neither of knots, defects of the wood, nor the very oblique direction of the lignous coats; it is perceived, that all these lignous pieces were rejected without the trouble of putting them to a trial. At length, from the numerous experiments which I made on these little pieces, I could only ascertain the results which I have here given, and I did not think it right to draw general confequences from them to form my tables on the refistance of wood.

These considerations, and the regret of lost labour, determined me to undertake experiments on the whole: I saw clearly the difficulty of the undertaking, but I could not resolve on myself to quit it, and, fortunately, I have been much more satisfied therein than I at first expected.

Experiment I.

I caused an oak of three seet circumserence, and about twenty-sive seet high, to be selled: it was strait, and without any branches to the height of sisteen or sixteen seet. I had it sawed down to sourteen seet, in order to avoid the desects of the wood, caused by the eruption of the branches; and afterward I had this piece sawed in the middle: each of these I had squared the next morning, and the ensuing day I had them reduced to sour inches square. These two pieces were very sound, and without any apparent knot: that which proceeded from the bottom of the tree weighed solb. that higher up, 56lb. To load the first, twenty-nine minutes of time was taken up, when it bent three inches and an half in

the middle before it cracked: the moment it cracked, we left off loading it, and it continued to crack and make much noise for twenty-two minutes; it bowed in its middle four inches and an half, and broke under the weight of 5350lb. The second piece, i.e. that which proceeded from the upper part, was loaded in twenty-two minutes: it bent four inches fix lines in its middle before it cracked; then we ceased loading it, and it continued cracking for eight minutes more, and bent fix inches fix lines in its middle, and broke under the load of 5275lb.

licable as that of the top. II

IN the same soil, I felled another like to the first, only fomewhat more lofty, though not fo thick, and its flem was pretty strait; many small branches of the fize of an inch appeared in the upper part, and at the height of about seventeen feet it divided in two thick branches. I had two pieces taken out of this tree, about eight feet long by four inches fquare, and I broke them two days after. The first, which proceeded from the foot of the tree, weighed 88 lb. and the seçond, taken from the upper part, weighed only 63lb. The first was loaded in fifteen minutes; it bent in its middle three inches nine lines before it cracked: as foon as it cracked, we left off loading of it, and it continued cracking for ten minutes, when it was bent eight inches in its middle, after which it broke with a great report under 4600lb. weight. The fecond was loaded in thirteen minutes: it bent four inches eight lines before it cracked, and after the first report it bent eleven inches in fix minutes, and then broke under 4500 lb. weight.

III.

THE same day I had a third oak felled, and had the trunk sawed in the middle. Two pieces were taken out nine seet long each by four feet square:

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Ii that

that of the foot weighed 77lb. and that of the top, 71lb. Having put them to the trial, the first was loaded in fourteen minutes; it bent four inches ten lines before it cracked, and afterwards seven inches and an half, and then broke under the weight of 4100lb. that above the trunk, which was loaded in twelve minutes, bent sive inches and an half, and cracked; afterwards it bent to nine inches, and broke quite through under the weight of 3950lb.

These experiments evince, that the wood of the foot of a tree is heavier than the wood of the trunk: they shew us also, that it is stronger, and not so

flexible as that of the top.

IV.

FROM the same quarter where I had taken the trees which I used for the preceding experiments, two oaks of the same kind, size, and nearly similar; their trunks were three seet round, and it was scarcely eleven or twelve seet high to the first branches. I squared them both, and took from each a piece of ten seet long by sour inches square; one of these pieces broke under the weight of 3625lb. and the second, under that of 3600lb. I must here observe, that an equal time was used in loading them, and that they split in sisteen minutes: the lightest bent a little more than the other, i.e. six inches and an half, and the other, sive inches ten lines.

V.

IN the same quarter I selled two more oaks of two seet ten and eleven inches in thickness, and the trunk about sisteen seet. From these I took two pieces twelve seet long by sour inches square: the first weighed 100lb. and the second, 98lb. The heaviest broke under the weight of 3050lb. and the other under 2925lb. after having bent in their middle, the first to seven and the second to eight inches.

These are all the experiments which I have made on pieces four inches square. I would not go farther than the length of twelve feet; because, in common use, builders and carpenters use very seldom only pieces of twelve feet by four inches square, yet it never happens that they make use only of pieces of sourteen or sisteen feet in length, and sour inches in thickness.

By comparing the different weight of the pieces used for the above experiments, we find by the first, that the cube foot of this wood weighed $74\frac{4}{7}$ lb. by the second $73\frac{6}{8}$ lb. by the third 74 lb. by the fourth $74\frac{7}{10}$ lb. and by the fifth $74\frac{7}{4}$ lb. which shews, that the cube foot weighed $74\frac{3}{10}$ lb.

By comparing the different loads on the pieces, with their length, we find, that pieces of seven feet in length supported 5313lb. those of eight feet, 4550lb. those of nine feet, 4025lb. those of ten feet, 3612lb. and those of twelve feet, 2987lb. whereas, by the general rule of mechanics, those of seven feet having supported 5315lb. those of eight feet ought to have supported 4649lb. those of nine feet, 4121lb. those of ten feet, 3719lb. and those of twelve feet, 3099lb. from whence it may be already supposed, that the strength of wood decreases more than in an inverted ratio of its length. As it appeared to me important to acquire an entire certainty on this circumstance, I undertook to make the following experiments on pieces five inches square, and of all lengths, from seven feet to twenty-eight.

VI

AS I was constrained to take all the trees destined for my experiments from the same soil, I was obliged to confine myself to pieces twenty-eight feet long, not having been able to find more losty oaks in that quarter; I chose two, the trunks of each was twenty-eight feet without thick branches, and upwards of forty to fifty feet high; these oaks were nearly five feet round the foot. I felled them the 14th of March, 1740, and had them squared the ensuing morning. A piece of twenty-eight feet long by five inches square, was taken from each tree: I examined them carefully in order to discover, if there were not some knots or defects of the wood towards the middle, but I found them to be very found. The first weighed 364lb. and the second, 360lb. I loaded the heaviest lightly. We began at fifty-five minutes after two; at three, it bent three inches in its middle, although it was not then loaded with 500lb. at five minutes after three, it bent feven inches, and it was loaded with 1000 lb. at ten minutes after three, it bent fourteen inches under 1500lb, weight; and, at last, at twelve or fifteen minutes after three, it had bent eighteen inches, and was loaded with 1800lb. at this instant the piece cracked violently; it continued cracking for fourteen minutes, and bent twenty-five inches; after which it broke through the middle under the aforementioned weight. fecond piece was loaded in the fame manner: we began at five minutes after four, and at first loaded it with 500lb. in five minutes it bent five inches; in the five following minutes it was loaded with soolb. more; it had bent eleven inches; in five more minutes it bent eighteen inches, under 1500lb. two minutes after, it cracked under 1750lb, when it had bent twenty-two inches. We then ceased from loading it: it continued cracking for fix minutes, and bent to twenty-eight inches before it broke entirely under the above load.

VII.

AS the heaviest of the two preceding pieces had broke right in the middle, and as the wood was neither

ther cracked nor split in the adjoining parts of the fracture, I thought that the two pieces of this broken one might serve for experiments on the length of fourteen feet: I forefaw, that the upper part of this piece weighed less, and broke easier, than the other which proceeded from the inferior part of the trunk; but, at the fame time, I perceived, that by taking the mediate term between the refistance of these two pieces, I should have a result, which would not be far distant from the real resistance of a piece of fourteen feet, taken from a tree about this height, I therefore had the remainder of the fibres. which still united the two parts, fawed; that which came from the foot of the tree weighed 185lb. and that from the top, 1781b. the first was loaded with a thousand weight during the first five minutes; it did not bend perceptibly under this load: we added a fecond thousand weight during the five following minutes; this weight bent it an inch in its middle: a third thousand, put on in another five minutes, bent it two inches; a fourth, bent it three inches and an half; and a fifth, to five inches and an half. We were going to continue loading it; but, after having added 250lb. to five thousand weight, it made a report; we then discontinued loading it, and the piece bent as far as ten inches in its middle, before it entirely broke under 5250lb. weight: it had supported all this weight for forty-one minutes.

We loaded the second piece as we had done the first, i.e. a thousand weight every five minutes. The first thousand weight bent it three lines; the second, one inch four lines; the third, three inches; the fourth, five inches nine lines: we loaded the fifth thousand, when the piece made a report all at once under 4650lb. weight, it had bent eight inches; after this first report, we ceased from loading it; the piece continued cracking for half an hour.

hour, and bent thirteen inches before it entirely

broke under 4650lb.

There are a great number of the like experiments, but as it might be looked on as tautology to repeat them, and as they are all performed partly in the fame mode only with respect to their different fizes, lengths and thicknesses; we shall not tire the reader with a tedious detail of them, as he will find them all repeated under a general view in the following tables.

TABLE of EXPERIMENTS,

On the STRENGTH of WOOD,

FIRST TABLE.

For Pieces of Four Inches Square.

Length of Pizczs.	Weight of Pirers.	Loaded with		The measure of the curvature of the piece in the inflant when they began to break:		
Feet.	Pounds:	Pounds.	Ho. Min.	Inch. Lin.		
7	60	5350 · · · 5275 · ·	0. 29			
8	68	4600	0. 15			
9	77	3950				
10	84		0. 15			
12	98	3050	0. 0	But the sales and		

SECOND TABLE.

For Pieces Four Inches Square.

Length of	Weight of	Loaded with	Time from the first re- ports to the moment of breaking.	Meefure of the curva- ture before fplitting.
Feet.	Pounds.	Pounds.	Ho: Min.	Inch. Lin.
7 {	94 · · · 88½ .	11775.		2. 6.
8 {	104	9900.	0. 40 .	2. 8.
9{	118	8400. 8325. 8200.	0. 28.	3. o. 3. 3. 3. 6.
10{	132 · · · · · · · · · · · · · · · · · · ·	7225 · 7050 · 7100 ·	0. 21.	3. 2. 3. 6. 4. 0.
12 {	156	6050 . 6100 .	0. 30.	5. 6. 5. 9.
14 {	178	5400.	0. 18.	8. 0.
16 }	209	4425 .	0. 17.	8. 1.
18 }	232	3750 · 3650 ·	0. 11.	8. o. 8. 2.
20 \$	263 · ·	3 ² 75 · 3 ¹ 75 ·	0. 10.	8. 10.
ASSESSMENT OF THE PARTY.	281	2975.	0. 18.	11. 3.
24 {	310 · ·	2200 . 2152 .	0. 16.	11. 10.
26				
28 {	364 · · 360 · ·	1800.	0. 17.	18

THIRD TABLE.

For Pieces Six Inches Square.

Length of	Weight of	Loaded with	Time from the first re- ports to the moment of breaking.	ture before
Feet.	Pounds.	Pound.	Ho. Min.	Inch. Lin.
7 {			1. 49	(1)
8 }			1. 12	
	166	13450	. 0. 56	2. 6.
io	188	11475	0. 46	3. 0.
. 0 1	224	9200	0. 31	4. 0.
14	255 -	7450	0. 25	4. 6.
i6	1294 .	6250	0. 20	. 5. 6.
i8	5 334 •	. 5625	0. 16	. 7. 5.
20	\$ 377 .	5025	. 0. 12 . 0. 11	. 9. 6.

⁽¹⁾ We have not been able to observe the quantity of pieces of seven inches bent in the middle by reason of the thickness of the iron.

FOURTH TABLE.

For Pieces Seven Inches Square.

Pizcas.	Weight of	Loaded with	Time from the first re- ports to the moment of breaking.	the curva-
Feet.	Pounds.	Pounds.	Ho. Min.	Inch. Lin.
7. 8	20.	67800	0. 0.	0. 9
85	204	26150.	2. 6.	
6	227	22800.	I. 40 . I. 37 .	3· I.
In :35	254	19650	I. 13. I. 16.	2. 7.
f2 S	302	16800	I. 3.	2. 11
14.05	351	13600	0. 55.	4. 2
16	406	11100	0. 48.	4. 10.
18	454	9450	0. 36.	5. 6.
20.		8550	0. 22.	7. 10.
	505	8000	0. 13.	8. 6.

FIFTH TABLE.

For Eight Inches Square.

Length of	Weight of	Loaded vith		Measure of the curva- ture before splitting.
Feet.	Pounds.	Pounds.	Ho. Min	Inch. Lin.
100	331	27800	2. 50	3. 0.
10	331	27700	2. 58.	2. 3.
			1. 30	
-	3951	-	Control of the Contro	
111	A		1. 6.	
	_		I. 2	
10	CONTRACTOR OF THE PARTY OF THE		0. 47	The second of the second
			0. 50	
			0. 32	A THE RESIDENCE
			0. 30	-
20			0. 24	
-	6601	12200	0. 20	0. 0

SIXTH TABLE.

The Medium of the Loads of all the preceding Experiments.

Length of		тн	CKN	E S S.	
PIECES.	4 Inches.	5 Inches.	6 Inches.	7 Inches.	8 Inches.
Feet.	Pounds.	Pounds.	Pounds.	Pounds.	Pounds.
7	5312	12525	18950.	the read library	STORM COMM
8	4550	97871.	15525 .	26050.	
9	4025	33081	13150.	22350.	100
10	3612	7125	11250.	19475	27750
12	29871	6075.	9100.	16175	23450
14	1.058	5300.	7475	13225	19775
16	025	4350.	63621	11000.	16375
18	1.5-851	3700.	. 5562 ½	9245	13200
20	41600	13225.	4950.	8375	11487
22	1.189	2975 .		116	
24	12.457	$2162\frac{1}{2}$	Tick of	1001.5	VIV. 5.
28		1775 .	1	- and grand	

SEVENTH TABLE.

Comparison of the resistance of wood, from the preceding Experiments, and of the resistance of wood according to the rule of this resistance, being as the breadth of the piece, multiplied by the square of the height, supposing the length to be the same.

ength of	Inches.	THIC	KNES	S. 7 Inches.	8 Inches.
Fcot,	Pounds.	Pounds.	Pounds:	Pounds.	Pounds.
	12.	250	18950	*32200	48100
1.5	ют.	11525	1199153	316243.	47198
0 7	550.	9787.	15525	26050 26856	*39750 · 40089}
0 6 .	$253\frac{13}{15}$	8308;	{ 13150 . { 14356 4	22350 22798	*32800. 34031.
	612.	} 7125.	£11250.	19475	27750. 29184.
17 2	987½ 110²	6075.	§ 9100 104973	and the second second	23450.
14		5100.	7475 88124	13225	00 -
16	• • •	4350.	§ 6362 9516		
18	• • •	3700.	{ 5562 6393		
20	111	. 3225 .	{ 4950 5572		. 11487 1

^{*} The afterifks mark that the experiments have not been made,

Drevented them from af-

An easy Mode to augment the Solidity, Strength, and Duration of WOOD.

FOR this purpose, it is only requisite to strip the tree of its bark from top to bottom during the time of its pith, and to suffer it to dry before it is felled. This preparation requires but very little expence. We shall point out the valuable advantages which accrue from it.

Matters as simple and as easy to be found out as this, have generally but a flight appearance in the eyes of physicians: but their utility is sufficient to render them worthy of being made public; and perhaps the perspicuity and care which I joined to my refearches, will make them meet with a favourable reception with those even, whose bad taste will only permit them to effeem a discovery which has cost much trouble and time. I own, that I am surprized to find myself the first to announce this, especially fince I have read what Vitruvius and Evelyn relates. The first informs us, in his Architecture, that before trees are felled, they should be tapt at the root, into the heart of the wood, and thus fuffer them to dry standing, after which they are much better for service. The second relates, in his Treatise on Forests, that Dr. Plot afferts, in his Natural History, that in Haffon in England, large trees are stripped of their bark standing about the time of their pith, and suffered to dry till the following year, when they are cut down: that they only fuffer the tree to live thus stripped of its bark, that the wood may become harder, and that they make use of the sappy part as well as of the heart. These circumstances are pretty exact, and related by authors of fuch great credit, as to have gained them the name of physicians, and even of architects: but there is all the reason imaginable to think, that, besides the negligence gence which has hitherto prevented them from affuring themselves of the truth of these circumstances, the sear of counteracting the forest laws,
has retarded their curiosity. It is prohibited,
under pain of great penalties, to strip a tree of its
bark and to suffer it to dry standing. This prohibition, which, in other respects, is well sounded, has
occasioned a contrary prejudice, which, without
doubt, has occasioned what we have related to be regarded as salse and hazardous; and I should mysels
still remain in ignorance, in this respect, if the attention the Count de Maurepas gives to the sciences,
had not procured me the liberty of making my experiments without searing to pay too dear for them.

- In a wood newly felled, and in which I referved some beautiful trees, the 3d of May, 1773, Istripped four oaks, thirty or forty feet high each, of their bark. These trees were all four very vigorous, with much pith, and about feventy years old. I stripped the bark from the fummit to the root of the tree. This operation is easy, the bark seperating very readily from the body of the tree in the time of the pith. These oaks were of the common kind, which bear the largest acorns. When they were entirely ftripped of their bark, I felled four more oaks of the same kind, in the same soil, and as like the first as I could meet with them. My defign was to ftrip fix more of their bark the fame day, and to fell fix more; but I could not finish this operation till the next morning. Of these fix oaks, only two were found to have less sap than the other four. I had these fix trees dried in their bark till I wanted them, in order to compare them with those I had stripped. As I imagined, that this operation had done them great injury, and would produce a great alteration, I went feveral days succesfively, to inspect them narrowly; but I perceived no fensible alteration for upwards of two months: at length, 901193

length, on the 10th of July, one of the oaks, which had least sap when stripped, shewed some symptoms of the disease which would soon destroy it. Its leaves began to sade on one side, and soon grew wholly yellow, dry, and began to fall off; so that by the 6th of August there was not one left. I had it felled the 30th of the same month, while I was present: it was grown so hard, that the hatchet difficultly entered it and broke: the sappy part appeared to be harder than the heart of the wood, which was still moist and full of pith.

That tree, which in the time of stripping off the bark, was no more pithy than the preceding, did not fail to follow it: its leaves began to change colour the 13th of July, and was entirely naked before the 10th of September. As I was fearful I had felled the first too soon, and that the humidity which I had remarked therein, indicated still some remains of life, I reserved that, to see whether it would shoot

out any leaves the fucceeding fpring.

My four other oaks vigorously held out, nor quitted their leaves till within a sew days of the customary time; and even one of the four, whose head was light and but little loaded with branches, only quitted them at the exact time of their natural fall; but I remarked, that the leaves, and even some of the whole four, were dried many days before.

In the ensuing spring, all these trees did not remain till the customary time for the development of the leaves: they were green eight or ten days before the season. I foresaw all what this effort must cost them. I observed the leaves, their growth was very quick, but presently stopped for want of sufficient nutriment; nevertheless, they lived; but the tree which the preceding year was stripped the first, also selt the first all the effect of inaction and dryness to which it was reduced: the leaves saded soon, and fell

fell in July, 1734. I had these selled the 30th of August, i. e. I judged that it was at least as hard as the other, and much harder in the heart of the wood, which was scarcely moist. I placed them by

the last, in order to compare them.

Three of the four remaining trees quitted their leaves the beginning of September; but the oak with a light head, retained them longer, and did not entirely part with them till the 22d of the same month: I reserved them for the following year, with three others which appeared to me the least difordered; and had two of the weakest felled in October 1734. I exposed two of these trees to the injuries of weather and time, and placed them by the others: they resisted greatly the wedge, and the heart of the wood was almost dry.

In the spring, 1735, the most vigorous of my two remaining trees afforded still some signs of life; the buds swelled, but the leaves could not develope: the other appeared perfectly dead. In fact, having felled them in May, I perceived, that there was no more radical moisture, and I found them to be very hard, as well outwardly as inwardly. I felled the last some time after, and placed it by the others, to

fubmit it to a new trial.

In order the better to compare the wood of trees stripped of their bark, with that of common wood, I took care to put these six oaks with an oak with its bark on, nearly of the same size; for I had already discovered by experience, that the wood of a tree of a certain size, was heavier and stronger than the wood of a smaller, although of the same age. I caused all my trees to be sawed into pieces sourteen seet long: I marked the centers above and below: at the ends of each piece, I had a square of six inches and an half sawed, and I had the four sides sawed off; so that there only remained of each of these pieces, a joist sour-

teen

teen feet long by fix inches square. I had them carefully reduced to this proportion in their whole length, and broke sour of each kind, in order to discover their strength, and to be well assured of the great difference which I at first found.

The piece taken from the body of the tree without bark, which had perished the first, weighed 242 lb. it was the weakest of all, and broke under 7940 lb.

That of the tree in bark, which I compared with

it, weighed 234lb. and broke under 7320lb.

That of the fecond tree without bark, weighed 249lb. it bent more than the first, and broke under 8362lb.

That of the tree in bark, which I compared with

it, weighed 236lb. it broke under 7385lb.

That of the tree without bark, exposed to the weather, weighed 258lb. it bent still more than the second, and did not break before it was loaded with 8926lb.

That of the tree in bark, which I compared with

it, weighed 239lb. and broke under 1420lb.

At length, that of the tree with a light head, which I judged the best, weighed, in fact, 263 lb. and bore, before it broke, 9046 lb.

The tree which I compared with it, weighed

238lb. and broke under 7500lb.

The two other trees without bark, were found defective in their middle, there being fome knots; fo that I would not have them broken. But the above trials are fufficient to shew, that wood stripped of its bark, and dried standing, is always heavier, and considerably stronger, than wood kept in its bark: what I am going to relate will leave no doubt thereon.

From the top of the trunk of the tree without bark, and left to the injuries of the weather, I had a piece taken out fix feet long and five inches fquare: on one of the fides, there was a small knot which pe-

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netrated scarcely half an inch; and, on the opposite side, a spot of an inch broad, of a browner wood than the rest. As these desects did not appear considerable, I weighed and loaded them; it weighed 75lb. it was loaded in one hour five minutes, with 8500lb. after which it cracked very violently: I thought it would have broke some time after it cracked, as it always happened; but, having patiently waited three hours, and seeing that it neither bowed nor, bent, I continued to load it; and, in another hour, it broke, after having cracked for half an hour, under the weight of 12745lb. I have only related this trial, to shew, that this piece of timber would have borne more, if it had not been for the little desects it had in two of its sides.

A fimilar piece of timber, taken from one of the trees with its bark on, weighed only 72 lb. it was very found, and we loaded it in one hour thirty-eight minutes, after which it cracked very flightly, and continued cracking for three hours, where it

broke under 11788lb.

This experiment is very advantageous, for it proves, that the wood above the trunk of a tree stripped of its bark, even with considerable defects, is heavier and stronger than the wood taken from the foot of another tree not stripped, which has otherwise no defect; but the following is still more favourable.

From the fappy part of one of my trees stripped of its bark, I took many bars, three feet long by one inch square; among which I chose five of the most perfect, in order to break them: the first weighed 23 ounces $\frac{5}{3^2}$, and broke under 287 lb. the second weighed 23 ounces $\frac{6}{3^2}$, and broke under 291½lb. the third weighed 23 ounces $\frac{4}{3^2}$, and broke under 275lb. the fourth weighed 23 ounces $\frac{28}{3^2}$, and broke under 291lb. and the fifth weighed 23 ounces $\frac{14}{3^2}$, and broke

broke under $291\frac{1}{2}$ lb. The mean weight is nearly 23 ounces $\frac{21}{32}$, and the mean load is nearly 287lb. Having made the fame trials on many bars of the fappy part of one of the trees with bark, the mean weight was found to be 23 ounces $\frac{2}{32}$, and the mean load 248lb, and afterwards, having also done the fame thing on many bars of the heart of the fame oak in bark, the mean weight was found to be 25 ounces $\frac{10}{32}$, and the mean load 256lb.

This proves, that the fappy part of the wood without bark is not only stronger than the common, but even much more so than the heart of an oak in bark, although less heavy. I must not here forget to remark, that I observed in all these trials, that the external part of the fap was that which refifted the most; so that it constantly required a greater load to break a bar of the fappy part, taken from the last circumference of the tree stripped of its bark, than to break a like bar from within. This is entirely contrary to what happens in trees treated in the common mode, the wood of which is lighter and weaker, in proportion as it is nigher the circumference. I determined the proportion of this diminution, by weighing, hydroftatically, pieces from the center of trees, pieces from the circumference of folid wood, and pieces from the fappy part: but this is not the place to relate this matter; I shall, therefore, content myfelf with observing, that in trees stripped of their bark, the diminution of the folidity of the center of the tree to the circumference, is not by far so sensible, nor is it so at all in the sappy part.

The experiments I have related are too numerous to allow of a doubt of the fact they tend to establish: it is, therefore, very certain, that the wood of trees stripped of their bark, and dried standing, is harder,

more folid, heavier, and stronger, than that of trees felled in their bark; and from hence, I think, it may be concluded, that it is also more durable, Immediate experiments on the duration of wood would be still more conclusive; but our own duration is so short, that it is not reasonable to attempt them: the brevity of our life feems to deprive us entirely of a great number of important truths; and we must leave to posterity experiments already begun, they will treat of them better than we have done; for the few physical traditions which our ancestors have left us, become useless for want of exactness, or from the little knowledge of authors, and the more from the hazardous circumstances, which they have not been ashamed of transmitting to us.

The physical cause of this augmentation of solidity and ftrength of wood deprived of its bark standing, offers of itself. It is sufficient to know, that trees increase in fize by additional coats of new wood, which is formed from the fap between the bark and the old wood. Our trees stripped of their bark, forms none of these new coats; and although they live after the bark is taken off, yet they do not grow. The substance destined to form the new wood, finding itself, therefore, stopped and obliged to fix in all the void places of the fappy part, and even of the heart of the tree, which necessarily augments the folidity, and must, consequently, augment the strength of the wood; for I have found, by many trials, that the heaviest wood is also the ftrongeft.

I do not think the explanation of this effect has need of being longer dwelt upon, but by reason of some particular circumstances which remain to be understood, I shall give the result of some other experiments which have a relation with this matter.

The

The eighteenth of December I raised the bark three inches from about three feet above the ground, from many oaks of different ages, so that the fappy part appeared naked: by this means I intercepted the fap, which would have paffed the bark to the wood: nevertheless, in the ensuing spring. these trees shot forth leaves like the rest, nor did I find any thing remarkable till the 22d of May. I then perceived fmall rolls about a line above the incision, which came from between the bark and the fap of these trees. Below the incision nothing appeared: in fummer, these rolls increased an inch descending and fixed themselves on the sap. The young trees formed rolls more extended than the old, and all preferved their leaves, which did not fall off at the usual time. In the ensuing spring they re-appeared a little before those of the other trees; I thought to have feen the rolls swell somewhat, but they did not extend at all: the leaves refifted the heat of fummer, and fell off but a few days before the rest. In the third spring, the trees were again adorned with verdure and advanced before the reft. but the youngest, or rather the smallest, did not remain long: the largest trees did not lose their leaves till Autumn, and I have had two which had fome after the fourth fpring; but most have died in the third or in the fourth year. I tried the strength of the wood of these trees, it appeared to be greater than that of the wood felled in common, but the difference which in wood entirely deprived of its bark is more than a fourth, is not by far fo confiderable here, and even is not firiking enough for me to relate the trials I made on this subject : and, in fact, these trees had not ceased growing above the incifion. There was only an expansion of the liber formed between the wood and the bark; fo, the fap, which in trees entirely stript of their bark, finding itself obliged to fix in the pores of the wood, and to deposits a small part of its substance in the internal part of the tree; the remainder was used for the formation of this unfruitful wood, whose rolls made the appendix and nutriment of the bark, which lived as long as the tree itself. Below the incision the bark lived also, but it formed neither rolls nor new wood, the action of the leaves and the upper parts of the tree pump up the sap too powerfully for it to be carried towards the bark of the lower part; and I imagine, that this bark of the foot of the tree, rather drew its nutriment from the humidity of the air than from that of the sap, which the vessels of the sappy part might furnish it with.

I made the like trials on feveral kinds of fruit trees; it is a certain means of hastening this production; they bloffom fometimes three weeks before the rest, and give early and pretty good fruit the first year. I have even had pears, the bark of the tree of which I have not only fripped but also the sappy part, and those premature fruit were as good as the others. I have also stript apple-trees of their bark from top to bottom; this operation killed the youngest of these trees the first year, but the larger have often Before the feafon withstood two or three years. came on, they were covered with a prodigious quantity of bloffoms, but the fruit which succeeded did not come to maturity, nor to a confiderable fize. I also endeavoured to re-establish the bark of trees, which is but too often raised off by different accidents, and I did not labour unsuccessfully; but this matter is quite different from that we make trial of, and requires a particular detail. I made use of the ideas which these experiments gave birth to.

I made the first essay on a quince tree, the third of April, I raised up the bark of two branches of this tree spirally, these afforded fruit, the rest of the tree shooted out very strongly and remained sterile; in-

flead

stead of raising up the bark, I have sometimes bound the branch or trunk of the tree with a small cord or piece of slax, the effect was the same, and I had the pleasure of gathering fruit from these sterile trees for a long time. The tree enlarging, did not break the band which bound it, it only formed two rolls the thickest above and the least below the packthread, and often after the first or second year, it was found covered and incorporated to the substance of the tree itself.

In whatever manner therefore the fap is intercepted, we are certain of hastening the production of trees, especially the blowing of flowers and the productions of fruit. I shall not give the explanation of this subject, we shall meet with it in the vegetable statues. This interception of the sap also hardens the wood, in whatever manner it is made, and the greater it is, the harder the wood becomes. In trees entirely deprived of their bark, the fappy part becomes not fo hard, as being more porous than the folid wood; it attracts the fap with more frength and in a greater quantity, the external fappy part of the tree pumps it up more powerfully than the in-The whole body of the tree attracts it until the capillary tubes are found to be full and obstructed. It requires a greater quantity of the fixed parts of the fap, to fill the capacity of the large pores of the thickest part of the tree, than to occupy the little interstices of solid wood, but the whole is filled nearly alike, and it is this, which is the cause of the diminution of the weight and strength of the wood in these trees, from the center to the circumference, is much less considerable than in trees cloathed with their bark; and this at the fame time proves, that the fappy part of these trees stript of their bark must not be looked upon as an imperfect wood, fince it has acquired in a year or two, by being stript, the folidity and strength which otherwise it would not have acquired under twelve or fifteen years: for it requires nearly this time in the best soil, to transform the sappy part in wood. It will not, therefore, be necessary to retrench the sappy part, as has till hitherto been done; and to reject it, we shall make use of trees of all sizes, since often we have found pieces in a foot of a tree, from which we could take only two. A tree forty years old, might serve for every use to which we put a tree of fixty years: in one word, this easy practice gives the double advantage of not only increasing the strength and solidity, but likewise the volume of wood.

But it will be faid, why has government prohibited the stripping trees of their bark with such great feverity? Would there not be some inconvenience in permitting of it, and does not this operation cause them to perish? It is true, an injury is done; but this injury is much less than is imagined; and befides, it acts only on the young, and is only perceptible in underwood. The views of government are just in this respect, and its severity is prudent. Timber merchants strip young oaks in their young state to fell the bark for tanning leather, which is the fole motive of stripping them of their bark. As it is more easy to strip off the bark when the tree is standing, than when felled, and that in this manner fewer workmen may make the same quantity of bark, and the custom of stripping the bark from the tree while standing would be re-established without the rigour of laws. Now, for a very trifling advantage, for a mode somewhat cheaper to strip the bark, we greatly injure the trees. In a quarter where I stripped off the bark, and dried the wood standing, I reckoned many which no longer shot forth, and a number of others which shot forth weaker than the general kind; their weakness was even less durable: for, after three or four years, I have feen their shoots scarcely equal the height of common shoots of

the

find-

the same age. The prohibition of barking the tree when standing is there founded upon reason, it is only requisite to make a few exceptions to this too

general rule.

YOL. V.

It is quite otherwise with respect to lofty trees than with the underwood, we should suffer the first and all useful trees to be barked; for we know that felled trees repel scarcely at all; that the older a tree is when felled, the less its exhausted shoot can produce; and so whether it is barked or not, the shoot of a useful tree will produce but little, when we shall have waited till the trees are aged before they are felled. With respect to trees of a moderate age, which generally leave the power of re-production to their shoots, the barking does not destroy it: for, having observed the shoots of my fix trees which were barked and dried standing, I had the pleasure to see four of them fpring out well, while the other two shot forth but very weakly; and these two, were those which, in the time of barking, were less in fap than Three years after barking, all these shoots the rest. were three or four feet high, and I make no doubt but that they would have been much higher if the shoots which surrounded them, did not deprive them of the influence of the air, so necessary to the growth of all plants.

This barking does not fo much injury to the shoots as might be thought: this fear, therefore, must not prevent the establishment of this easy and very advantageous custom: but we must restrain it to trees destined for service, and chuse the six trees of the greatest sap to perform this operation; for then the canals are more open, the strength of suction greater, the liquors slow more easily, pass more freely, and, consequently, the capillary tubes preserve their power of attraction longer, and all the canals do not close till a long time after barking; whereas, in trees barked before the sap, the road for the liquors not

Mm

finding itself rubbed, and the most common road finding itself broken before it has been made use of, the fap cannot so easily obtain a passage, the greatest part of the canals do not open to receive it, its action to penetrate is impotent, and these tubes cut off from all nutriment, are obstructed by defect of tension; the others never open fo much as they would do in the natural state of the tree, and at the arrival of the fap, they present only finall orifices, which, in fact, must pump up with great strength; but which must always be rather filled and obstructed, than the open and diffended tubes of the trees which the fap has moistened, and prepared before barking. is the reason, that in our experiments, the two trees which were not fo much in fap as the rest, perished the first, and that their fuccours had not the power of re-production; we must, therefore, wait for the time of the greatest fap to bark trees. We shall alfo, by this attention, gain a very great facility to perform this operation, which in no other time would not be fo long, and which, in this feafon of the fap, becomes a very small work, fince one man alone on the top of a high tree, may bark it from top to the bottom in less than two hours.

I have not had occasion to make the same trials on other woods as on the oak; but, I do not doubt the barking and drying when standing, renders all wood more compact and close; so that I think we cannot too greatly extend and recommend this prac-

tice.

ARTICLE II.

EXPERIMENTS on the drying of WOOD in AIR, and on its Inebibition in WATER.

EXPERIMENT I.

To know the Time and Gradation of Drying.

THE 22d of May, 1773, I felled an oak about ninety years old; I had it fawed and fquared on the spot; and cut out a block in form of a paralleliped of fourteen inches two lines and an half half high, eight inches two lines thick, and nine inches five lines broad: I was reduced to these proportions, because I would not make use of the perfect wood, called the Heart, and from which I had exactly taken up the sappy part of white wood. This piece of the heart weighed at first 45lb. 10 ounces, which comes nearly to 72lb. 3 ounces the cube foot.

TABLE of the drying of this Piece of Wood.

YEARS, M				ood.	YEAR	s, Mon	THS,	and Da	YS.	WEI-	7 7 7 7 7 7 7 7 7 7 7 7 7 7 7 7 7 7 7 7
	-	-	1b.	oz.	No. February	COME THRESTON	CHATS.		MALLAN.	lb.	0 z
1733. M	ay	23.	45.	10	1733.	Sept.	26			36.	1
			1	1		Oct.	26,	dry		35.	5
		25.	44.	10		Nov.	3,	dry		35.	44
		26.	44.	5			17,	rain		35.	
		27.	44.	14		Dec.	Ι,	rain		35.	4
11111111		28.	43.	113			15,	frost		35.	31
1000		29.	43.	74			29,	moist		35.	
		30.	43.	4	1734	Jan.	12,	varia	ble	35.	
3	une		42.		1			frost		35.	1 1
1		6.	146.	1	1	Feb.	9,	rain		35.	
		10	44.	. 6	1		23,	wind	y	35.	. 3
		14	. 40.	14.	1-01	Mar.	9,	tempe	erate	34.	154
		18	. 40.	. 7				rain			154
		26	. 39	. 15	1	April	26			34.	10
J	uly	4.	. 39	. 8	1	May	26			34.	7
		16	. 38	. I 2	1	June	26			33.	14
		20	. 38	. 6	1	July	26			33.	61
A	ug.	. 26	. 37	. 3	100	Aug.	. 26			133.	

YEARS, MONTHS, 2nd DAYS.	WEIGHT of Wood.	YEARS, MONTHS, and DAYS:	WEIGHT of Wood.	
1734. September 26. October 26. November 26. December . 26. 1735. January . 26. February . 26. March 26.	1b. oz. 32. II 32. 7 32. II 32. I2 ¹ / ₂ 32. I2 32. I2 32. I3 32. 8 32. 7 32. 6 32. 4 32. 4	1775. November . 26. December . 26. 1736. February . 26. May 27.	1b. oz. 32. 3 32. 5½ 32. 1 32. 3 31. 10½ 31. 5¼ 31. 5¼ 31. 1½ 31. 1½ 31. 1½	

This table, as we see, contains the quantity and proportion of drying for ten years. After the seventh year, the dryness was complete. This piece of wood, which at first weighed 45 lb. 10 ounces and an half: lost in drying 14 lb. 8 ounces, i. e. even a third of its weight. It may be remarked, that it required seven years for its complete drying; but that in eleven days it was a quarter dry, and in two months almost half dry; since on the 2d of June it had lost 3 lb. 9 ounces, and on the 20th of July, 1733, it had lost 7 lb. 4 ounces, and at last it was three quarters dry in ten months. We must also observe, that as soon as this piece was about two thirds dry, it retook as much and even more humidity than it exhaled.

EXPERIMENT II.

To compare the Time and Gradation of Dryness.

THE 22d of May, 1734, from the fame tree, I had fawed a block, from which I took a piece perfectly like the first, and exactly reduced to the same dimensions. This trunk of the tree was exposed

exposed to the weather a year: it was left in its bark, and to prevent its decaying, care was taken to turn the trunk from time to time. This second piece of wood was rather a little above the first.

TABLE of the drying of this Piece.

YEARS, A	IONT	is, ar	d I	DAY	ş.	WEI of W	ood.	YEARS, MONTHS, and DAYS.	WE1	ood.
	-	_	_			lb.	oz.		ib.	oz.
734. May	23,	at 8	o'cl	ocl	cm.	42.	8	1735. Jan. 26.	35.	21
	24, 1	8 01	m.			42.		Feb. 26.	35.	1
		to 8				41.	121	Mar. 26.		
		8 01				41.	de			
		ditto				41.	61	May 26.	34.	5
	27					41.	3	June 26.		
	28					40.	-	July 26.		
	29					1	13	Aug. 26.		
	30						III		32.	14
June	-					40.	7 }			
3	6					40.	I	Nov. 26	132.	15
	10				10	4	101			
	14							1736. Feb. 26		
	18	•				39.	-			
	26		•			38.		•		
July		•	•	•				1737. Feb. 26		
July	16		•	•		37.		1738. ditto 26		
	26		•		•			1739. ditto 26		
Aug.	¥ .					37· 36.		1740. ditto 26		
Sept.			•			-		1741. ditto 26		
Oct.				•		35.				
				•		35.		1742. ditto 26		-
Nov.				•		35.		1743. ditto 26		
Dec.	20					35	4	1744. ditto 26	131	: 4

By comparing the preceding table with the first, we find, that in one year the wood in the rough was no drier than the wood which was worked was in eleven days. We likewise see, that it required eight years for the whole drying of this piece of wood which had been preserved in its bark for a year; whereas, the wood worked at first was found entirely dry in seven years. I suppose, that this piece of

wood weighed as much, and perhaps a little more than the first; and that when it was rough, and the tree just felled, the 23d of May, 1733; that is to fay, it weighed then 45lb. 10 or 12 ounces. This supposition is well founded, because this piece of wood was cut and worked in the same manner exactly on the fame dimensions, and at the end of fix years, after its perfect drying, the difference was found to be only three ounces, which is a very small difference, and which I attribute to the folidity or denfity of the first pieces, because, the second was taken immediately below the first from the foot of the tree: now, it is well known, that the nearer we approach the root of the tree, the denfer the wood With respect to the drying of this piece of wood, after it has been worked, we find it required feven years to dry it perfectly like the first piece: that it required twenty days to dry this fecond piece a fourth; two months and an half to dry it half, and thirteen months to dry it three-fourths. At length, we find it was reduced, like the first, to about two thirds of its weight.

It must be remarked that this tree was in sap when it was cut the 23d of May 1733, and that consequently the quantity of the sap is formed by this experiment to be one third of the weight of the wood, and that there is only two-thirds solid and liguous parts, and one-half liquid parts, as we shall find in the course of these experiments. This dryness and considerable loss of weight has altered nothing of its volume. The two pieces of wood have also the the same dimensions, so that the sap is lodged in the interstices of the leginous parts of these interstices remain void, and the same after the evaporation of

the humid parts which they contain.

We have not observed that this wood although cut in full sap, was worm eaten; it was very sound and the two pieces were

EXPERIMENT III.

To know whether the dryness is made proportionable to the surfaces.

THE eighth of April 1732, I had two pieces of oak cut at the same time, one in the form of a paralleliped and the other in form of small planks of an equal thickness. Seven of these small planks weighed as much as the first piece, and the superficies of this first piece was to those of the planks nearly as to is to 34.

TABLE of the Proportion of Drynes's.

Months and Days,	Weight of the eight pieces.	of the feven	Months and Days.	Weight of the eight pieces.	Weight of the feven pieces.
1734. April.	grains.	grains.	1734.	grains.	grains.
8 to 2 in ev.		2189	Ap. 26, dry		1479
8 to 10 ev.		1981	27, dry		1458
	2070	1851	28, dry		1449
10 o'clock.	,	1	29, win.		1447
	1973	1712	30, rain		1461
11	1 200	1628	May. 1. wet		1468
12		1589	5, rain .		1478
13, fair wea-		1,	9, fair .		1475
	17781	1565	13, wet .		1476
14, dry		15401	21, fair .		1465
15, dry		1525	29, wind		1 ,
16, dry		1518	and rain	1	1466
17, dry,		1505\$	June 6, rain		1489
18, dry		1502	July 6, fair		1479
19, cloudy .		14971	Aug. 6, dry		1468
20, wet	-	1493	10, dry	1 -	1461
21		1486	12, dry .		1450
22. var		1481	14, dry .		1448
23, hot		1485	15, dry .		14603
24		1486	16, rain		1468
25, dry		1482	17, fair .		1450

Before we examine the refult of this experiment, we must observe, that it required 492 of the grains I made use of to make an ounce, and that the cube foot of this wood weighed nearly 66lb. That the piece I made use of weighed nearly seven cubical inches, and that the furfaces were as ten to thirtyfour. By confulting the table we find, that the drying in the first eight hours was for the fingle pieces of fifty-nine grains, and for the seven pieces of 208 grains, fo that the proportion of the drying is greater than that of the furfaces, for the piece losing fiftynine, the feven pieces should have lost only 2001. Afterwards we see, that from ten o'clock in the evening to feven in the morning, the fingle piece loft fixty grains, and the feven pieces loft 130, and that confequently the drying which at first was too great, in proportion to the furfaces, is at prefent too little, because it should have had for the proportion to be just, that the fingle piece losing fixty, the feven pieces should have lost 204, whereas it only lost 130.

By comparing the following time, i. e. the fourth part of the table, we perceive, that this proportion diminishes very considerably, insomuch that the seven pieces lose only very little in comparison of their surface, and from the fifth term, the single piece is found to lose more than the seven pieces, since its dryness is ninety-three grains, and that of the seven pieces is only eighty-four. Thus dryness is here made at first in a somewhat greater proportion than that of the surfaces, afterwards in a smaller, and at length it becomes greater as the surface is smaller. We perceive that it requires only sive days to dry the seven pieces so much that the single piece should

We also perceive, that it requires only twentyone days to give a perfect dryness to the seven pieces, fince on the twenty-ninth of April, they weighed no more than 1447 grains and a half, which is the

greateft

greatest degree of lightness they required, and that in less than twenty-four hours they were half dry, whereas the single piece was not perfectly dry in four months and seven days, since it was the 15th of August when it was at its greatest degree of lightness. It weighed then only 1461 grains, and that in three days it was half dry. We also find that the seven pieces lost by the drying more than one-third of this weight, and the single piece near a third.

EXPERIMENT IV

THE 26th of February 1744, I exposed to the fun two pieces of wood, which served me for the first experiment, and which I kept for twenty years. The oldest of these pieces, i. e. that which had served me for my first experiment, weighed 31 lb. 1.02. 2 drams, and the other 31 lb. 4 oz. They had been first dried in the air for ten years, and afterwards expeled to the fun from the 26th of February to the 8th of March, and always kept from the rain. They dried still, and the first weighed no more than 30 lb. 50z. 4 drams, and the second, 30 lb. 6 oz. 2 dr. To dry them still the more, I put them both into an oven, heated to 47 degrees above the freezing point; it was 40 min. after nine in the morning, and we drew them from the oven two hours after; we measured them exactly, and their dimensions did not change perceptibly. I only remarked, that there were feveral cracks on the four fides, and that the longest was about a line: we weighed them when they came from the oven; the first piece weighted only 29lb. 6 oz 7 dr. and the fecond 29 lb. 6 oz. At that moment I threw them into a great veffel filled with water, and loaded each piece with a stone to keep it at the bottom of the veffel.

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TABLE of the Imbibition of the two Pieces of Wood which were entirely dry when plunged into Water.

MONTHS AND DAYS:		Time the Wood re- mained in the Oven and Water.	of	GHT the of Woon.
2744-	014	All the All the	g 4311/2	B. oz. dr.
March 8 .		(-10-1-1	ift.	30 5 4
9.	• •	put in the oven at 9 h. 40 min. and at 11 h. 40 min.	rft.	29 67
	V	put in waterat 11h.	19,00	29 67
	•	40 min. after 11, and drawn out 40 min, after noon.	ift.	32 0 2
9.	. :	r hour	{ ift.	32 8 6 33 4 6
9.		r hour	{ rft. 2nd.	32 13 6 33 9 I
tom tegs / 9 ·		t hour	{ ift, 2d.	33 I 3 33 I3 I
9.		t hour,	fift.	33 3 4 34 3 9
9.		; hour . : .	f ift.	33 6 o 34 I 7
9.	• •	1 hour 15 mg.	{ 1ft. 2nd.	33 8 o
adition 9.	• •	1 hour 45 m.	f ift.	33 9 T 34 5 2
9 •		1 hour 55 m.	1 ft. 2nd.	33 16 4 34 6 6
9.	• •	1 hour 55 m.	{ 1st. 2nd.	33 II 4 34 7 2
9 .		r hour	{ 1st. 2nd.	32 13 2
9 .	710	r hour	{ ift. 2nd.	33 13 6
10		rr hours	fift.	34 6 6 35 2 6
10		12 hours	fift.	34 11 2

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MONTHS AND DAYS.	mained in	Time the Wood re- mained in the Oven and Water,			WEIGHT of the Precess of Wood.		
1744.	1				lb. oz. dr.		
March 11	ra hours			rft.	35 0 0 35 12 1		
i : .	12 hours			ift.	35 3 t		
12	12 hours			ift.	35 14 I 35 6 5		
i2	r 2 hours			rft.	36 2 6 35 9 3		
13	12 hours			and.	36 5 3		
13	1 2 hours			and.	36 7 6		
14	1 2 hours			and.	36 10 1		
14	i 2 hours	11.		and.	36 13 1 36 3 1		
15	12 hours			2nd.	36 15 0 36 4 6		
15	12 hours			and.	37 0 7 36 6 2		
16	12 hours		. }	2nd.	37 2 2 36 8 1		
16	12 hours		. }	and.	37 3 4 36 9 0		
17	12 houts		. {	and.	37 5 3 36 10 2		
ī7	12 hours		. {	and.	37 6 0 36 11 2		
18	12 hours		. }	2nd. 1ft. 2nd.	37 7 3 36 12 6		
18	12 hours		. 1	1ft. 2nd.	37 8 4 36 13 2		
ig	12 hours		. 1	ift.	37 9 4 36 14 7		
19	12 hours			ift.	37 10 7 37 0 2		
	12 hours	2	. }	ift.	37 12 2 37 1 1 37 13 6		

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MONTHS AND DAYS.		Time the Wood re- mained in the Oven and Water.			WEIGHT of the PIRCES of WOOD.		
1744.						1	b. 02. dr.
March.	. 40		12 hours		. {	ıst.	37 20
						and.	37 14 3
	21	•	12 hours	•	:	and.	37 3 7
	4.7		12 hours	1		Ift.	37 15 2
	31		12 nours	•		and.	37 3 6 38 0 7
	22		12 hours			ıft.	37 4 5
						and.	38 1 4
	22		12 hours			Ift.	37 5 2
	1.00					2nd.	38 2 4
	23		24 hours			Ift.	37 6 4
1 01 0	Fr. St.					2nd.	38 3 2
S 1 3	34		24 hours			Ift.	37 7 7
I II II	. hallen	1				land.	38 50
2	25		24 hours			Ift.	37 9 2
0 33 3	533					land.	38 6 6
9 1 6 -	26		24 hours			f tft.	37 10 3
40 75	Sile					l 2nd.	38 7 5
10 30	27		24 hours			f ift.	37 11 3
2 2 12	. 0	4				l and	38 8 7
2 9 00.	28		24 hours		•	Ift.	37 12 2
4 15 11		1	24 hours			I and.	38 10 0
0 6 95	29		24 Hours		•	and.	37 13 1
0 8 10	30.		24 hours			f ift.	38 10 3
THE RE	34	3	24	•	•	and.	38 11 3
0.0	31		24 hours			f ift.	37 14 3
* 11	3,					and.	38 11 5
April .	1		24 hours			I ift.	37 14 7
V MA TE		1	1 1000			land.	38 12 4
3 2 1	2		24 hours			fift.	38 0 1
			. FEEL.		1	land.	38 13 1
	3		24 hours			Į ift.	38 0 6
* 0 * 1				0.1		l and.	38 14 0
40 00	4		24 hours			} ift.	38 1 2
1 21 7	1	1				21111.	38 14 2
12 2 -	.5		24 hours			} rft.	38 1 7
6	La					¿ 2nd.	38 15 1

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MONTHS Time the Wood mained in the Orand Water.		the Ore	n of	GHT the of Woods
1744-				lb. oz. dr.
April 6, rais	n 24 hour		{ 1ft. 2nd.	38 3 0
7, rain	24 hours		frft.	38 3 3
8, rain	. 24 hours		S ift.	38 3 6
9, rair	. 24 hour		} ift.	39 1 2 38 4 6
to, rain	a . 24 hour		5 1st.	39 1 5 38 5 1
II, rai	n . 24 hour		2 nd.	39 2 f 38 6 7
12, col	d . 24 hour		3 1ft.	39 3 4 38 7 5
13, dr	y - 24 hour		3 1st.	39 50
14, col	d . 24 hour	s	S rft.	39 6 4 38 9 6
15, rai	a . 24 hour	rs	S rft.	39 6 6 38 10 2
16, wir	nd . 24 hour	rs	frit.	39 7 4 38 10 7
17, rai	n . 24 hour		2nd.	38 11 4
18, fai	r . 24 hou	rs .	. { 1 st.	38 12 I
19, ra	in . 24 hou	rs .	. 1 1ft.	38 13 I
20, ra	in . 24 hou	rs .	. [1st.	38 13 2
21, fa	ir . 24 hou	rs .	. 5 rft.	38 14 0
22, fa	air . 24 hor	ore .	2nd	38 14 6
23, Wi	ind . 24 hor	irs .	. 5 rit.	38 15 6
24, F	ain . 24 ho	urs .	\frac{2 nd}{2 nd}	39 0 3

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MONTHS AND DAYS.	AND Time the Wood remained in the Oven and Water.					
1744-					n.	
April. 25, rain.	24 hours			ıft.		1 5
- 26, dry .	24 hours			1ft. 2nd.	39	16
27, wind .	24 hours			ift.	39	30
28, rain .	24 hours			ift.	39	4 1
29, fair .	24 hours		•	ift.	40 39 40	1043
30, dry .	24 hours			fift.	39	50
May 1, fair .	24 hours			1 ft. 2 nd.	39	60
z, hot .	24 hours			{ fft. 2nd.	39	6 4
3, fair .	24 hours			1 ft. 2 nd.	39	6 7
4, fair .	24 hours	• .		1 1ft. 2 nd.	39	3770
5, fair .	24 hours			{ 1st. 2nd.	39	47
6, wind .	24 hours			1 ift. 2 nd.	39	7 4
7, rain .	24 hours			{ ift. 2nd.	39	75
8, rain .	24 hours			{ ift. 2nd.	39	5 3 8 5
9, fair .	24 hours			{ 1st. } 2nd.	39	5 3 9 2 6 0
11, wind	2 days			{ ift. 2nd.	39 40	9 1
13, wind	2 days			{ ift. 2nd.	39	5 3 9 3 5 6
15, wind	. 2 days			{ 1st. 2nd.	39	9 7
17, rain	2 days			fift.	39	5 7 10 5 6 3

MONTHS AND DAYS.	Time the Wood re- mained in the Oven and Water.		AND Time the mained and Wa		of	GHT the of Woon.
1744.						lb. oz. dr.
May 19, rain		days	•	. {	ift.	39 11 5
21, thur	1.	2 days		. {	ift.	39 12 5 40 8 3
23, fair		a days	2 4	. {	ift.	39 13 3
25, rain		2 days			ıst.	39 14 4 40 10 0
27, fair		2 days			Ift.	40 I I 40 I2 3
29, fair		2 days			1ft. 2nd.	40 2 0
31, fair		2 days	ab s		{ ift.	40 1 2
June 2, dry	IS OP	2 days	ido	50	f ift.	40 2 4
4, rair	1.	2 days	•		1 ft. 2 nd.	40 4 I 40 14 I
6, dry		2 days			fift.	40 5 0
8, dry	y .	2 days			{ 1st. 2nd.	45 5 0
10, dr	y .	2 days			f ift. 2nd.	40 5 6
12, .		2 days			fift.	40 6 5
14, ho	t.	2 days			1st. 2nd.	40 7 2
16, rai	in .	2 days			fift.	40 8 3
18, clo	ud.	2 days			fift.	40 10 1
20, ra	in .	2 days			f ift. 2nd.	40 10 4
22, clo	oud.	2 days			fift.	40 11 5
34, he	ot .	z days			{ ift. 2nd	40 11 7

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MONTHS AND DAYS,	Time the Wood re- mained in the Over and Water.	
1744.		lb. oz.dr.
June . 26, dry	. 2 days	{ 1st. 40 13 0 2nd. 41 6 2
28, dry	. 2 days	{ 1st. 40 13 3 2nd. 41 6 5
30, dry	. 2 days	{ 1st. 40 14 6 7 2nd. 41 6 7
July . 2, hot	A CALL S	{ 1st. 40 14 1 2nd. 41 7 0
4. rain	· 2 days · ·	{ 1st. 40 15 3 2nd. 41 8 5
6; rain	· 2 days	{ ift. 41 0 4 2nd. 41 8 7
8, wind	. 2 days	{ 1st. 41 10 0
The roth we w		ange, the two cir-
12, rain		[ift. 41 26
1 4 04 011	2 days	2nd. 41 10 6
16, rain	. 4 days	fift. 41 4 1
20, rain	. 4 days	2nd. 41 12 0 1st, 41 5 0
24, clou	d. 4 days	2nd. 41 13 0 { ift. 41 6 6
28, fair	. 4 days	2nd. 41 4 5 1st. 41 8 4
Aug 1, wind	d. 4 days	1 st. 41 9 4
5, clou	d. 4 days	1 2nd. 42 1 0
9, hot	. 4 days	2nd. 42 2 3
13, rain	4 days	2nd. 42 3 2 1st. 41 12 1
17, wine	d . 4 days	2nd. 42 3 7 1st. 41 12 7
21, rain	0: 4 days .	l 2nd. 42 5 3 . { 1st. 41 13 5 2nd. 42 5 4

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MONTHS AND DAYS.	Time the V mained in t and Water.	he Oven		he
1744.			lb.	oz. dr.
Aug 25, var	4 days	!	1fc. 4	1 14 7
29, fair .	4 days		rit. 4	2 6 7
Sept 2, fair .	4 days		rit. 4	2 7 2
6, fair .	4 days			2 8 0
10, var.	4 days			2 9 2
14, fair .	4 days			2 10 5
18, hot .	4 days			2 11 4
22, fair .	4 days) .	2 12 0
26, hot .	4 days			2 11 6
30, fair .	4 days		(.	2 12 2
October 4, wind.	4 days		1	12 13 1
8, rain .	4 days		C .	12 14 2
12, rain	4 days		2nd.	12 14 2
16, rain		,	and.	12 15 0
20, rain			2nd.	43 0 3
			l and.	43 1 3
24, rain	1 - 11		2 20d.	42 12 0
28, fnow			l and.	42 12 2
Nov 1, fair			1 .	42 12 6 43 3 2
5, rain	· 4 days		Eit.	42 13 2

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MONTHS AND DAYS.	Time the Wood re- mained in the Oven and Water.		WEIGHT of the Pieces of Wood.		
Caracteria concessor	-			-	-
1744.			1		1b. 02. dr .
Nov 9, fair.	4 days		. {	ıst.	42 14 0
in the			1	and.	43 4 6
13, fair .	4 days		. !	ıst.	42 14 4
17, rain .	4 days		. 1	ıft.	43 5 2 42 15 2
1,,	4 443		. !	and.	43 5 6
21, var .	4 days			ıft.	43 0 2
	1.		- 1	2nd.	43 6 2
25, fair .	4 days		. !	ıft.	43 1 9
				2nd.	43 7 0
29, fnow	4 days			ist.	43 2 0
& frost.				2nd.	43 8 0
Dec 3, frost.	4 days			ıst.	43 2 2
				2nd.	43 8 2
7, var	4 days			ıst.	43 2 6
				2nd.	43 8 4
11, frost.	4 days			ıst.	43 3 9
				and.	43 9 0
15, rain	4 days			ıft.	43 2 6
& fnow.				and.	43 9 6
ro, rain	4 days			ıít.	43 3 4
& hail.				and.	43 9 4
23, rain	8 days			Ift.	43 3 5
& fnow.	e dama			2nd.	43 10 0
31, fnow	8 days	•		1 ift.	43 5 0
& froft.				cand	43 10 6
1745. Jan 8, hail	8 days			f ift.	
Jan 8, hail & rain .	o days			and.	43 5 4
16, frost.	4 days			fift.	
io, non .	4 days			and.	43 7 4 43 13 0
24, froft *	8 days			f ift.	43 13 0
24,11011	days	•		and.	43 14 0
Febr 1, fnow .	8 days			fift.	43 7 7
. cini 1, mow .	, o any			and.	43 15 4
					100

^{*} The water was entirely frozen; there was only one pint of water which was not in ice: we changed the wood two days after.

AND	Time the Wood remained in the Oven and Water.		WEIGHT of the PIECES of Wood.
1745.			
Feb 9, rain .	8 days		16. oz.dr. 118. 43 8 3 2nd. 43 15 3
17, rain wind & fn.	8 days		{ ift. 43 8 3
27, fair.	8 days		1st. 43 9 6
March. 5, fair b	8 days		2nd. 44 1 0 { ift. 43 11 4
13, frost.	8 days		2nd. 44 4 0 1st. 44 12 2
21, wind.	8 days		2nd. 44 50 { ift. 43 11 0
29, fair .	8 days		1 st. 44 3 1 0 2 nd. 44 3 2
April . 6, dry .	8 days		{ 1st. 43 11 2
14, dry .	8 days		12nd. 44 3 3 1st. 43 13 4
22, rain .	8 days		2nd. 44 50 [1st, 43 13 0
30, fair .	8 days		\ \ \frac{11 \text{ft.}}{33 \ \ \ \ \ \ \ \ \ \ \ \ \ \ \ \ \ \
May . 8, rain .	8 days		2nd. 44 5 3 1st. 43 14 3
16, fair,	8 days	:	2nd. 44 7 2 1st. 43 15 0
rain. 24, hot,	8 days		2nd. 44 7 0 { 1st. 44 1 0
June 1, cold	8 days		\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\\
9, fresh		. :	12nd. 44 8 7
br. & ho 17, frest wind	8 days	. :	\\ \text{1st. 44 9 4} \\ \{ \text{1st. 44 2 0} \\ \}
Willia	•		2nd. 44 9

b The wood was so strongly enclosed by the ice, that it required hot water. They were left near the kitchen chimney all night, and weighed twelve hours after the hot water was put into this copper.

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MONTHS AND DAYS.	Time the Wood remained in the Oven and Water.				
1745.			lb. 02. dr.		
June . 25, rain & wind.	8 days		Ift. 44 3 4 2nd. 44 11 1		
July 3, rain, hot.	8 days		{ 1st. 44 3 4 2nd. 44 11 1		
11, var	8 days		Ift. 44 4 6 2nd. 44 11 2		
19, rain,	8 days		{ Ift. 44 5 5 2nd. 44 13 0		
27, fair .	8 days		Ift. 44 6 6 2nd. 44 12 0		
Aug 4, rain .	8 days		Ift. 44 7 4 2nd. 44 13 4		
12, rain.	8 days		{ 1st. 44-83		
20, rain .	8 days		{ Ift. 44 9 0		
28, rain,	8 days		{ 1ft. 44 10 i		
Sept 5, fair .	16 days		lift. 44 10 4		
21, fair .	16 days		1st. 44 11 6		
Octob 7, dry .	16 days		Ift. 44 13 1		
23, fair .	16 days		2nd. 45 5 7 1st. 44 15 6 2nd. 45 6 1		
Nov 8, var	16 days		{ Ift. 45 1 4		
24, wet.	16 days		1.6 45 40		
Dec 10, frost.	16 days		\\ \text{ift. 45 46} \\ \text{and 45 10 }		
26, wet .	16 days	. :	S rft. 45 5 0		
24, wet. Dec 10, frost. 26, wet. 1746. Jan 11, var.	16 days		{ ift. 45 4 4 4 2nd. 45 9 0		

MONTHS AND DAYS.	Time the Wood re- mained in the Oven and Water.		WEIGHT of the Pieces of Wood		
1746.					lb. cz.dr.
Jan 27, frost,	16 days .			ıft.	45 68
rain:				and.	45 12 0
Feb. 12, rain,	16 days			ıft.	45 6 4
fnow.				2nd.	45 12 0
28, frost .	16 days			Ift.	45 80
			6	2nd.	45 12 4
March 16, frost.	16 days			I ift.	45 90
				2nd.	45 13 0
April . 1, wind	16 days			ıft.	45 90
& fnow.	1 2 2			2nd.	45 13 0
17, dry.	16 days			Ift.	45 10 0
May . 3, var .	1			2nd.	45 14 0
19, dry	16 days			ıft.	45 10 0
& hot.				land.	46 00
June . 4, rain .	16 days			fift.	45 9 4 45 14 2
20, var.	16 days	1		fift.	45 10 6
July . 6, var.	16 days			Ift.	45 10 5
hot.	1.			and.	46 0 1
22, dry .	16 days	•	•	and.	45 10 5
Aug 7, wet .	16 days	•	•	{ 1ft. 2nd.	45 12 0
23, hot .	16 days			f ift.	45 15 3
,				land.	46 2 5
Sept 8, rain .	16 days			S ift.	45 15 6
	1			land.	46 3 0
24, dry .	16 days			f ift.	46 0 6
	1			1 and.	46 3 6
Octob 10, wet .	16 days			Į ift.	46 1 3
	1			l 2nd.	46 4 3
26, fair .	16 days			S ift.	46 1 0
				land.	
Nov 11, var.	16 days			{ rft.	46 2 6
	1			l and.	
27, frost	16 days			{ 1st. 2nd.	46 3

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	Time the Wood re mained in the Ove and Water.	
1746.		lb. oz. dr.
Dec 13, wet .	16 days	{ rst. 46 4 4 2nd. 46 7 4
29, wet •	16 days	{ 1ft. 46 3 0 2nd. 46 7 0
1747. Jan 14, frost .	16 days	f ist. 46 30
30, wet .	16 days	\\ \begin{array}{cccccccccccccccccccccccccccccccccccc
Feb 15, temp.	16 days	l 2nd. 46 7 0
March . 3, frost.	16 days .	\\ \text{2nd. } 46 6 0 \\ \text{1ft. } 46 3 0
19, cold.	16 dáys .	2nd. 46 8 0 1st. 46 2 8
April. 4, rain.	16 days .	2nd. 46 8 8 1st. 46 5 1
20, dry	16 days .	2nd. 46 9 5 1st. 46 4 7
May 6, temp.	16 days .	2nd. 46 8 1.
22, var	· 16 days .	land. 46 9.4 . [1st. 46 75]
June . 7, rain .	16 days .	2nd. 46 9 0 1st. 46 8 2 2nd. 46 10 3
23, temp. & rainy		. { 1st. 46 9 1 2nd. 46 12 1
July . 9, var.		. { 1st. 46 10 0 2nd. 46 13 0
25, hot &	16 days .	. { 1st. 46 12 0
Aug 10, hot 8	16 days .	. { 1st. 46 11 0 2nd. 46 13 2
26, hot 8	16 days .	. { 1st. 46 12 0 2nd. 46 15 0
Sept 11, dry		. { Ist. 46 11 0

NATURAL HISTORY. 295

MONTHS AND DAYS.	Time the Wood re- mained in the Oven and Water.		0	I G H T
7747.				lb. oz. dr.
Sept. 27, rain	16 days		f ift. 2nd.	46 11 o 46 13 4
Oct 27, fair .	30 days		f ift. 2nd.	46 12 0
Nov 27, cloud.	30 days		f ift.	46 14 0
Dec 27, rain .	30 days		f ift.	47 0 4 46 15 0
1748.			and.	47 1 7
Jan 27, frost, fn. & hail			1 ift.	47 0 0
Feb 27, mild	30 days		ift.	47 I O 47 2 4
March . 27, cold	30 days		Ift.	47 0 4 47 4 0
April . 27, cold & rain	30 days		Ift.	47 2 0
May 27, dry & cold	1		{ ift.	46 20
June . 27, dry.	30 days		lift.	46 14 0
July . 27, heat	30 days		and.	46 16 2
& rain August . 27, heat	30 days		and.	47 2 I 47 2 O
Sept 27, rain.	30 days		and.	47 4 0
Oct 27, wet .	30 days		and.	47 5 5 47 7 3
Nov 27, frost	30 days		\ 2nd. \ \ 1st.	47 7 4 47 4 I
Dec 27, rain & fn.	30 days		l 2nd.	47 7 4 47 4 4
& fn.			L 2nd.	47 6 7
2749. Jan 27, rain	30 days		ſ rft.	47 6 4
2	130 days	•	and.	47 7 4

ig6 NATURAL HISTORY.

MONTHS AND DAYS.	Time the Wood remained in the Over and Water.	
1749.		lb. cz. dr.
Feb 27, rainy very dry	30 days	Ift. 47 6 0 2nd. 47 8 2
Mar 27, rain .	30 days	Ift. 47 8 0
Apr 27, wind .	30 days	2nd. 47 9 4 1st. 47 7 0 2nd. 47 9 0
May . 27, heat	30 days	Ift. 47 60 2nd. 47 8 0
June. 27, var	30 days	{ ift. 47 6 4 2nd. 47 8 0
July . 27, var	30 days	{ 1st. 47 7 2 2nd. 47 8 2
Aug. 27, rain .	30 days	{ 1st. 47 10 0 2nd. 47 11 0
Sept. 27, dry .	30 days	{ Ift. 47 8 0 2nd. 47 10 0
Oct. 27, dry	30 days	\{\text{ift. 47 6 o}\\ \text{2nd. 47 7 o}\end{array}
Nov. 27, rain	30 days	{ 1st. 47 12 0
Dec. 27, frost & hail	30 days	{ 1st. 47 14 0 2nd. 47 14 0
1750. Jan. 27, wet .	30 days	[rst. 47 15 0
Feb 27, var	30 days	1 2nd. 47 15 4 { 1st. 47 15 4
March 27, fair .	30 days	2nd. 47 15 6 { ift. 47 14 0
Apr 27, dry .	1	1 2nd. 48 2 0 1 1st. 47 12 4 2 nd. 47 13 4
May 27, rain .	30 days	{ 1st. 47 14 0 2nd. 47 15 0
June . 27, fultry .	30 days	{ 1st. 47 13 4 2nd. 47 13 4
July . 27, hot .	30 days	{ Ift. 47 13 0 { 2nd. 47 14 0

NATURAL HISTORY. 297

MONTHS AND DAYS.	Time the V mained in t and Water.	WEIGHT of the Pizces of Wood,				
1750.				16.	oz. dr.	
August . 27, rain	16 days		1 ft. 2 nd.	48	00	
Sept. 27, fult	30 days		ift.	48	10	
Oct 27, fair & cloud.	30 days		1 ft. 2 nd.	48	1 0	,
Nov 27, rain	30 days		fift.	48	20	,
1751 *.						
Jan 27, rain	61 days		{ ift.	48 48	10 0	
Feb 27, frost	30 days		{ ift.	48	90	•
March . 27, rain	30 days		{ ift. 2nd.	48	13 0)
April . 27, rain	30 days		{ ift.	48 48	13 0	•
May 27, var.	30 days		{ ift. 2nd.	48	13 0	•
July . 27, heat	30 days		{ ift. 2nd.	48	8 0	0
Aug 27, temp.	60 days		fift.	48	7 0	0
Oct 27, rain.	60 days		{ ift. 2nd.	49	0 6	•
Dec 27, fro@	60 days		{ ift. 2nd.	48	10 6	•
1752.				7		1
Feb 27, var.	60 days		{ rft.	48		
Apr '27, dry	60 days		{ ift. 2nd.	48		0

We forgotto weigh the two pieces in December.

MONTHS AND DAYS.	e Oven	WEIGHT of the Pieces of Wood,		
1745.				lb. 02. dr.
June . 27, hot & rainy			ist.	48 8 0 48 8 0
Aug 27, var.	60 days		f ist.	48 10 0
Oct 27, fair .	60 days		Ift.	48 10 0
Dec 27, rain .	60 days		ift.	48 11 0
1753.			- 211d.	40 12 0
Feb 27, mild	60 days		fift.	48 10 4
Apr 27, rain	60 days		f tit.	48 11.6
			Land.	48 12 0

By this experiment which lasted twenty years, we see:

First. That after drying in the air for ten years, and afterwards in the sun and fire for ten days, the oak attained its perfect dryness, and loses about one-third of its weight when worked green, and one-half less when kept in its bark a year before it is worked; for the piece of the first experiment was in ten years reduced from 45lb. 10 oz. to 29 lb. 6 oz. 7 drams, and the piece of the second experiment was in nine years reduced from 42 lb. 8 oz. to 29 lb. 6 oz.

Secondly. That the wood kept in its bark before it is worked, more readily and abundantly imbibes water, and confequently the humidity of the air than green wood. For the first piece which weighed 29lb. 6 oz. 7 drams, when put in water, in one hour gained only

weighed 27 lb. 8 oz. whereas the fecond piece, which weighed 27 lb. 6 oz. gained at the fame time 3 lb. 6 oz. This difference was kept up for some time, for in twenty-four hours stay in water, the first piece only gained 4 lb. 15 oz. 7 drams, whereas the second gained during the same time 5 lb. 40z. 6 drams. During eight days the first piece gained only 7 lb. 1 oz. 2 dr. whereas the second weighed 7 lb. 12 oz. 2 drams. In a month the first piece gained only 8 lb. 12 oz. whereas the second gained 9 lb. 11 oz. 2 drams. In three months the first piece gained only 10 lb. 14 oz. 1 dram, whereas the second gained 11 lb. 8 oz. 5 dr. At length the two pieces did not become of equal weight untill four years 7 days.

Thirdly. That it was twenty months before these pieces of wood at first so dried to the last degree, had imbibed in the water as much humidity as they had when standing, and at the moment when the tree was felled, for after twenty days stay in water, they weighed 45lb. some ounces, nearly as much as when they

were worked off.

Fourthly. That after twenty months stay in water, having imbibed as much humidity as they had at first, the water continued to ooze from the wood for sive years; for in October 1751, they both equally weighed 49 lb. therefore wood plunged in water, not only takes in as much humidity as it contains sap, but also upwards of one-sourth more, and the difference in weight from the persect dryness to the compleat imbibition, is from thirty to sitty, or from three to sive. A piece of very dry wood, which weighed only 3 lb. will weigh 5 lb. when it has remained several days in water.

Five. When the imbibition of wood in water is compleat, the wood at the bottom of the water follows the vicifitudes of the atmosphere, it was always heavier when it rained, and lighter when it was fair, as we have observed in the table. So that it may justly

justly be said to be moister in water when it rains, than when it is fine weather.

EXPERIMENT VIII.

To discover the difference of the imbibition of wood, as the solidity is greater or less.

THE 2d April, 1735, I took three cylinders from an oak fixty years old, one was from the center of the tree, the fecond from the circumference of the wood, and the third from the fappy part; these three cylinders weighed 985 grains each. I put them into a vessel filled with soft water, and weighed them every day for a month, to see the proportion of this imbibition.

7 ABLE of the Imbibition of the Cylinders of Wood,

	TES		INDE	
	GHING.	Heart.	Circumf.	
1735.		grains.		grains,
April.	2: 2 o'cl.	985.	985.	985.
	3 a 6 m.		1016.	1065.
	4	1021.	1027.	1065
	5, rain .	1023.	1034.	1073 .
	6, rain .		1040.	1081.
	7, rain .	1035.	1044.	1083.
	8, rain .	1036.	1048.	10881.
	9, rain .		1051.	1090
	10, cloud.		1055.	10927
	11, dry .		1056.	1084.
	12, dry .	1042.	1059.	1078.
	13, dry .		1061.	10787.
	14, cloud.		. 1064.	1079 .
	15, dry .			1078.

DATES		GHT		
WEIGHING.		Circumf. of Heart	Sappy	
1735.	grains.	grains.	grains.	
April. 16, wet .	1051.	1066.	1074.	
17, wet .	10511.	1067.	1072.	
18, dry .	1052.	1068.	1073	
19, dry ·	1053.	1069.	1071.	
20, cloud.	1056.	1072	1072	
21, rain .	1057.	1073.	1079.	
22, cloud.	10571	10751.	10781.	
23, cloud.	1058.	107 7	1074.	
24, dry .	1059.	10781.	1074.	
25, dry .	1060.	1079.	1074.	
29, dry .	1065.	1087.	10742.	
May 5, wet .	10681		1071.	
9, dry.	1072.	1093.	1071.	
13, wet .	1073.	10952	1070.	
21, rain .	1075.	1101	1070.	
25, rain .				
June., 2, dry.		11034.		
10, dry .		1108.	10781	
18, dry .		1105.	1064.	
July . 6, rain .		1109.	1069.	
15, rain .		1112.	1077.	
25, rain .		1126.	1098.	
Aug 25, dry .		1122.	1065.	
Sept 25, rain .		1126.	1092.	
October 25, rain.	11 128.	11130.	1024	

This experiment prefents fomething very irregular; we fee, that for the first year, this sap, which is the heart solid of the three pieces, imbibes 80 grains of water, when the piece of the circumference of the heart imbibes only 31, and the piece of the center 28; that also the next morning this sappy piece ceased from imbibing, so that its weight encreased only 1 grain, during 24 hours; whereas the other pieces imbibed and encreased in weight. By casting our eyes on the table, we perceive, that the center piece and that from the circumference, encreased in weight from the

2d April to the 10th June, whereas the fappy piece augments and diminishes in very irregular variations. It was put in water the 1st of April at noon, in cloudy and wet weather, this piece weighed like the two others 185 grains; the next morning at ten o'clock, it weighed 1065 grains; fo that in 18 hours it had encreated 80 grains, i. e. 12 of its total weight. was natural to think that it would continue to encrease in weight, nevertheless at the end of 13 hours it ceased fuddenly from imbibing. Afterwards this fappy piece imbibed water again, and continued fo to do for fix days, infomuch that by the 10th April, it had imbibed 1071 grains of water, but the two following days, it loft 14 grains, which makes more than 1-half of what it had done the fix preceding days; it remained almost at the same station during the three following days, after which it continued to give out the water it had taken in, infomuch that on the 19th of the fame month, it had given out 211 grains from the It diminished still more to the 13th and 21st of the fame month, and still more to the 18th of June, for it loft 28 grains from the 10th of April. After that it encreased during the month of July, and by the 25th it had imbibed in the whole 113 grains. During the month of A guft, it regained 33 grains, and at length it encreased in September, and especially so considerably in October, that by the 25th it had gained in all 139 grains.

From this and a number of the like experiments, it is therefore very certain, that wood plunged in water, attracts it and rejects it alternately, in a proportion, the quantities of which are very confiderable with refpect to the total imbibition. This circumstance assonished me, I at first imagined, that the variations might depend on the weight of the air; I thought that the air being heavier in dry and hot weather, the water being then charged with a greater weight, must penetrate the wood with a greater

force

force, and that on the contrary, when the air is lighter, the water which entered therein by the great weight of the atmosphere, might ooze out again; but this explanation does not agree with observations; for on the contrary it appears, that wood always increases its weight in water in rainy weather, and diminishes considerably in dry and hot weather, which made me propose to M. Dalibard, some years after, to make these experiments, by comparing the variations of the weight of the wood, with the motions of the barometer, thermometer, and hygrometer, which he executed with success.

EXPERIMENT IX.

On the imbibition of green wood.

THE 9th of April 1775, I cut a piece of cylindrical wood, which weighed 8 oz. from the center of an oak fixty years old: I immediately immerged it in a veffel full of water, which I always took care to keep filled.

TABLE of the imbibition of this piece of the heart of oak *.

YEARS, MONTHS AND DAYS.	WEIGHT of the Heart of Oak.	YEARS, MONTHS AND DAYS.	
1735.	ounces	1735	ounces.
April 9.	II.	April 22.	II .
10.	11	25.	11 .
11.	11 .	29.	II .
12.	II .	May 5.	II .
13.	11	13.	II .
14.	II.	29.	II .
15.	11 .	June 14.	II .
16.	11 .	30.	11 .
17.	II .	July 25.	II .
18.	11	April 25.	II .
19.		Septem. 25.	
20.	II	October. 25.	12 .
21.	11		12 .

^{*} The water, although very often changed, received a black colour, a fhort time after the wood was immerged therein; this water was fometimes covered with a kind of oily pellicle, and the wood was confantly gluish untill the 29th of April, although the water clarified itself a few days after.

By this experiment it appears, that there is an unctious matter in wood, which water readily diffolves; it appears also, that there are parts of iron in this matter, which gives the black colour to the water.

We see also, that the wood just felled, does not increase so much in weight in water, since in six months the augmentation is only one-half of the whole weight.

EXPERIMENT X.

On the imbibition of dry wood, both in salt and fresh water.

THE 22d of April 1775, I cut two small parallel pieces, of an inch square by two inches from a piece of oak, twenty years old, and which had been always covered; prior to this I had disfolved an ounce of sea-salt in 15 oz. of water, after having set down the pieces of wood, which was 450 grains each, I put one of them into the salt-water, and the other into a like quantity of fresh, they were immerged at sive o'clockin the evening, and suffered to float freely therein.

The TABLE of these two Pieces of wood.

AND	of the wood	WEIGHT of the wood in falt water,
DAYS.		
1775	grains.	grains.
April 22 at 7 o'cl. eve.	485.	grains. 481.
at 10 in the morn.	495.	487.
23 at 6 in the eve.	5061.	495.
at 6 in the morn.	5211.	502.
24 at 6 in the eve.	5311.	5091.
25	547	5172.*
26	560.	528.
27 at 6 in the morn.	573.	533.
28	582.	5392
29	5891.	5454.
30	598.	549
May . Ift	603.	551.
2	6091.	5534.
5	628.	585.
9 · · · · · · i	6481.	597 -
13	667.	607.
17	682.	616.
21	684.	620.
29	704.	630.
June . 6	7121.	640.
14	732.	648.
50	7532.	6631.
July 25	770.	710.
Aug. 25	7821.	736.
Sept. 25	7881.	7561.
Octo. 25	7963.	760.

* Small chrystals of falt were found about this piece, a little below the line of the water in which it floated.

I observed in the course of this experiment, that the wood became more glossy and oily in the fresh than in the salt water, the fresh water became also blacker. By this experiment we find, that the wood attracts a greater quantity in fresh than in salt water, which we shall be convinced of by the following table:

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The same day 22d April, I took fix pieces of an inch square from the same piece of oak, each weighed 460 grains. I put three into 45 ounces of water with 3 ounces of salt, and put the three other into 45 ounces of fresh water. I marked them 1, 2, 3, which were in the salt water, and 4, 5, 6, in the fresh.

* They were put in the water at half an hour after five in the evening)

MONTHS	1	1
	Weight	Weights
AND	of the	of the
DAY S	1 2 3.	4 5 6.
-	-	-
1735.	grains.	grains.
April. 22 at 6 o'lock &	450.	454.
an half.	4493.	452.
	4481.	451.
at 7 o'clock	£453.	459.
& an half.	452.	458.
W 111 11111	451.	4551
at 8 o'clock	(456.	463.
& an half.		462.
C an man	453.	4591:
at o o'clock	(458:	466.
& an half.	457.	465.
all hall.	C 433.	462.
22 at 6 o'clock in	(467.	4791:
the morning.	404.	476.
the morning.	(463.	475.
at 6 in the	c 475.	494.
evening.	474.	491.
evening.	471.	488-
24, at the fame	(482.	505.
hour.	480.	503.
nour.	479.	501.
	490분.	518.
25	486,.	519.
ent sold sons aw 51 m	4851.	413:
the flerit change in the	501.	532.
26	497.	529:
	495.	527.

M	ON	TI	IS	-	1	The state of	1
						Weight	Weight
	4	ND			-	of the	of the
D	A	Y	S.		1	4 5 6.	1 2 3.
					_	-	
1735.		8				grains.	grains.
,,,,					(507.	545.
April	2	27			. 3	524.	540.
					(499.	539:
					(514.	555:
		28			. }	509.	552:
					1	505.	551:
					(517.	560:
		29			. }	513.	557:
,		1			i	507.	555-
						522.	571.
		30			.)	520.	568.
					1	512.	567.
					(527.	575:
May.		I.e	r.		. }	525.	571:
						515.	560.
				-1-	.1. (530.	582:
		at				529.	577.
	111	the	eve	nin	S.	519.	575:
					(567.	600.
		5			. <	564.	594:
					(555.	593:
					. (573-	621.
		9				570.	613:
					-	561.	606.
						581.	634.
		13			,	578.	632.
						570.	624.
						1 589.	653:
		17				582.	648.
						575.	637.
						1 597-	670:
		21				584.	655:
						583.	649.
						619.	682.
		29				618.	667.
						612.	664.
June .	6	at	60	'cle	oct.	1622.	664:
June .		the				620.	680:
	214		CYC	-1111	2.	613.	679:

MONTH AND DAYS	s.			Weight of the numbers 1 2 3.	of the
1735.				grains.	grains.
June : 14	•			628. 627. 620.	703: 699: 691:
30		:		645. 642. 634.	274: 715: 713:
July . 25	•	:		663. 657. 648.	737. 731. 729:
August . 25			•	688. 694. 686:	747· 742: 736:
September 25				718. 711. 704.	752: 741: 740:
October				723.	757- 751.

From this and the preceding experiments we gather,

That oak loses about one-third of its weight in drying, and that less solid wood than oak loses more than one-third of its weight

2. That it requires at least seven years to dry timber eight or nine inches thick, and that consequently it would require much more than double the time, to dry beams sixteen or eighteen inches square.

3. That wood felled and kept in its bark dries flowly, that the time it is so kept is entirely loss, and consequently wood should be squared a short time after it is felled.

4. That when wood has acquired two-thirds of its dryness, it begins to pump out again the moisture of the

the air, and that confequently it ought to be preferved in inclosed places, if designed for joiners use.

5. That the drying does not remarkably diminish its volume, and that the quantity of the sap is one-

third of the folid part of the tree.

6. That the wood of the oak felled in full fap, if it is without that foft part of the timber between the body and bark of the tree, is no more subject to worms

than the oak felled in any other feafon.

7. That the drying of wood is at first in a greater ratio than that of the surfaces, and afterwards in a less. That the total drying of piece of wood of equal volume and of a double the surface of another, is made in twice or thrice less time; that the total drying of wood of equal volume and treble surface is made in five or fix times less time.

.8. That the augmentation of the weight which dry wood acquires by re-pumping the humidity of the

air, is proportionable to the furface.

9. That the total drying of wood is in proportion to its lightness, so that the sappy part dries more than the heart of the oak, as the ratio of its relative distance, which is nearly \frac{1}{15} less than that of the heart.

That when the wood is entirely dried in the shade, the quantity that still may be dried in the sun, and afterwards in an oven heated to 47 degrees, will scarcely be \(\frac{1}{17}\) or \(\frac{1}{8}\) part of the total weight of the the wood, and that consequently this artificial drying is expensive and useless.

in water, is filled in a very short time: that it requires, for example, but one day for a small piece of this sappy part to be filled with water, whereas it requires more than twenty days for a like piece of the heart.

12. That the heart of oak, increases only ¹/₁₀ of its whole weight, when immerged in water as soon as cut

cut, and that a long time is required even to gain this augmentation of 1.

13. That the wood immerged in foft water, at-

tracts it more readily and abundantly than in falt.

14. That wood immerged in water imbibes much quicker than it does in the air, fince it only required twelve days for the two first experiments to re-take in water half of the moisture it had lost by drying in feven years, and that in twenty-two months they are loaded with as much humidity as they ever had, infomuch, that at the end of those twenty-two months stay in water, they weighed as much as when they

had been cut twelve years before.

15. At length, when the wood is entirely filled with water, they undergo at the bottom of the water variations relative to those of the atmosphere, and which are discovered by the variation of their weight, and although it is not perfectly known to what these variations correspond, we nevertheless see in general, that wood immerged in water is damper where the air is damp, and less so when it is dry, since it constantly weighed more in rainy weather than when fair.

NATURAL HISTORY

AND

THEORY

OF THE

E A R T H.

Translated from the FRENCH

Of COUNT de BUFFON.

Intendant of the Royal Gardens in France; Member of the French Academy, of the Academy of Sciences, and of the Royal Societies of London, Berlin, &c.

By W. KENRICK, L.L.D. and OTHERS.

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Vide ego, quod fuerat quondam solidissuna tellus,

Este fretum; vidi fractas ex æquore terras;

Este procul à pelago conchæ jacuere marinæ,

Et vetus inventa est in montibus anchora summis;

Quodque fuit campus, vallem decursus aquarum.

Fecit, & eluvie mons est deductus in æquor.

Ovid. Metam. lib. 15:





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THEORY of the EARTH.

E Hall not here speak of the figure of the w we earth, nor its motion, nor the connections it may have with the rest of the universe. It is its internal constitution, its form and its matter, which we now propose to examine. The general history of the earth, is a necessary study for those who defire to make themselves acquainted with nature and her productions, and the detail of fingular circumstances of the life and manners of animals, or of the culture and vegetation of plants, belong perhaps, less to natural history than to the general results of observations made on the different matters which compose the terrestrial globe; the eminences, depths, and inequalities of its form, the motion of the fea, the direction of mountains, the position of quarries, the rapidity and effects of currents, &c. This is nature in its ample extent, and these are her principal operations; they influence all the rest, and the theory of these effects is a first science of which the intelligence of particular phenomena, as well as the exact knowledge of terrestrial substances depends; and when we give to this part of the natural sciences the name of physics, or physic, in which we admit no system, is it not the history of nature.

Vol. V. 2 R



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One aftronomer more ingenious than reasonable, and versed in the system of Newton, foreseeing every possible event of the cause and direction of the planets; explains by the assistance of a mathematical calculation, or by the tail of a comet, every alteration hap-

pened to the terrestrial globe.

Another heterodox theologician; his brain heated with poetical visions, thought he had discovered the creation of the universe, and assuming a prophetic style, after telling us what the earth was before it came from Chaos, what the deluged had changed therein, and what it has been and what it is; predicts what will happen, even after the destruction of the human race.

A third; In fact, a better observer of nature than the two first, (though all but little regular in their ideas,) explains by an immense liquid, contained in the bowels, the principal phenomena of the earth, which according to him, is only a superficial and very thin crust, which serves as a covering to the fluid it incloses.

All these hypothesis's formed at random, and which are only built on ruinous soundations, have given no light to ideas and have consounded truth. Fable is mixed with physics, and these systems have been received only by those who blindly receive every thing, and are incapable to distinguish the links of probability, and are more struck with the marvellous than the truth.

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What we have to fay on the subject of the earth, will be without doubt less extraordinary, and appear common in comparison of the great systems before mentioned; but, it must be remembered, that an historians duty is to describe, not to invent that no supposition must be admitted, and that he must make use of his imagination only to combine observations, generalize facts, and form a collection which may present to the mind a methodical order of clear ideas, and of successive and probable connections; I say probable, for we must not hope to be able to give exact demonstrations on this matter; they have place only in mathematical sciences, and our knowledge in physics and natural history depends on experience, and is confined to inductions.

Let us therefore begin by reprefenting what the experience of time and our own observations has taught us on the subject of the earth. This immense globe offers at the furface, acclivities, depths, plains, feas, lakes, rivers, caverns, gulphs and volcanos, and all this at the first inspection, discovers to us no regularity or order. If we penetrate into its internal part, we shall there find minerals, stones, bitumen, fand, earth, water and matters of all kinds; placed as it were by chance without the least apparent regularity; by examining it with more attention, we fee mountains which are funk, rocks split and broken, countries swallowed up, new islands, others under water, and caverns filled up. We shall find the heaviest matters placed on the lightest, hard bodies furrounded with foft, matters which are dry, wet, hot, cold, &c. and all mixed in a kind of confusion, which prefents to us no other image than a mass of ruins and a wrecked world.

Nevertheless we inhabit these ruins with a perfect security. Generations of men, animals and plants, succeed without any interruption, the earth surnishes subsistance in abundance; the sea has its li-

mits

mits and laws, its motions are subjected, the air has its regulated currents, the seasons their periodical and certain returns, the verdure never fails to succeed the hoary frost; all appears in order, and the earth, which but a short time ago, was only a chaos, is a delightful abode, where calmness and harmony reigns, where all is animated and conducted, by a power and intelligence, which fills us with admiration, and raises us to the sublime idea of

an Almighty and wife Creator.

Let us therefore, not lay any firefs on the irregularity on the furface of the earth, and on the apparent disorders found in the interior part; for we shall soon perceive the utility, and even the neceffity of it; and by confidering it further, we perhaps shall find there an order, and general connections which we did not perceive at the first glance. In fact, our knowledge in this respect will be always confined; we do not yet know the whole furface of the globe, we are partly ignorant of what is at the bottom of the sea, and there are parts, the depth of which we have not been able to fathom, we can only penetrate into the coat of the earth, and the greatest cavities, and the deepest mines do not descend to the hundredth part of its diameter, we can therefore judge only of the external and almost superficial state; for the internal part is almost entirely unknown to us. We know that, volume for volume, the earth weighs four times more than the fun, we have also the rotation of its weight with other planets, but it is only a relative estimation, the unity of proportion is wanting, the real weight of matter being unknown to us, infomuch that the internal part of the earth may be either void, or filled with matter, a thousand times heavier than gold, which we have no model to discover, nor can we scarcely form any reasonable conjectures there-

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We must therefore confine ourselves to the examination and description of the surface of the earth. and the most internal thickness to which we have penetrated. The first thing which presents itself, is the immense quantity of water, which covers the greatest part of the globe: this water always occupies the lowest parts, always level, and perpetually tends to an equilibrium and rest; nevertheless, we see it agitated by a strong power, which oppofing its tranquility impresses it with a periodical and regular motion, raifes and lowers the waves alternatively, and forms a counterpoise of the total mass of the sea, by disturbing it to the greatest depth. We know that this motion has been from eternity, and will remain as long as the moon, and the fun which caused them.

By afterwards confidering the bottom of the fea, we shall remark there as many inequalities as on the furface of the earth; we shall find there eminences, valleys, plains, depths, rocks, and foils of all kinds, we shall see that all islands are only the summits of vast mountains, the feet and roots of which are covered with the liquid element; we shall there find fummits of other mountains, which are near the top of the water, we shall there remark, rapid currents, which feem to be different from the general motion, we shall see them sometimes retrogade never exceeding their bounds, which appear as invariable as those, which confine the rivers of the earth. In one part are stormy countries, where the wind precipitates the tempests furiously, and where the sea and heaven equally agitated, strike against, and confound each other; these intestine motions, boilings, drummings, and extraordinary agitations, caused by volcanos, the mouths of which, although buried under water, yet vomit fire from the midst of the waves, and

fend up to the clouds a vapour mixed with water, sulphur, and bitumen. Farther I see gulphs we dare not approach, and which seem to attract vessels to ingulph them: beyond I perceive vast plains ever calm and tranquil, but quite as dangerous, where the winds have never exercised their power, where the art of the mariner becomes useless, and where he must remain and perish. At last casting my eye to the extremities of the globe, I see enormous slakes of ice, which loosened from the continents of the poles, come like mountains, floating and melting to the more

temperate regions.

These then are the principal objects which the vast empire of the sea affords us, and thousands of inhabitants of all kinds, people all the extent, Some covered with light scales traverse the different countries: others loaded with a thick shell drag themselves securely along, and mark by their slowness, their track in the fand: others, to whom nature has given fins in form of wings, make use of them to raise and support themselves in the air: and others, to whom all motion has been refused, grow and live attached to rocks, &c. and all find their food in this element. The bottom of the fea produces in great abundance plants, mosses, and still more fingular productions; the foil of the fea is fand, gravel, and often mud, and fometimes folid earth, shells, rocks, and every where resembles the earth we inhabit.

Let us now travel on the dry part of the globe, what a prodigious difference of climate? What variety? and what inequality of foil? But let us minutely observe it, and we shall discover, that the greatest chain of mountains is found nearest the equation than the poles, than in the old continent, they extend from east to west, much more than from north to south, and that in the new world, on the contrary,

contrary, they extend from north to fouth, much more than from east to west; but what is very remarkable, is, that the form of the mountains, and their circumference, which appear absolutely irregular, have nevertheless, corresponding directions, to that the faillant angle of one mountain, is always opposite to the returning angle of the neighbouring mountain, separated from it, by a valley or depth. I observe also, that opposite hills have nearly the fame height, and that in general, mountains occupy the middle of continents, and divide in the greatest length, islands, and other high earths. I follow the direction of the longest rivers in the ame manner, and find, that they are always almost perpendicular to the course of the sea in which their mouth is inserted, and in the greatest part of their courses, they proceed nearly as the chain of mountains, from which they take their securest direction. By afterwards examining the shores of the sea, I find that they are commonly confined by rocks, marble, and other hard stone, or by earth and fand, which has accumulated of itself, or which the waters have brought thither; and I remark, that neighbouring coasts, which are only seperated by an arm of the fea, are composed of the like matters, and that the beds of the earth, are the same in the one, as in the other. I fee that volcano's are all found in the highest mountains, that there are a great number, whose fires are entirely extinct, that some have fubterraneous correspondences, and that their explofions are made fometimes at the fame time. perceive a fimilar correspondence between certain lakes and neighbouring feas: in one place, are floods and torrents, which lose themselves all at once, and precipitate themselves into the earth; there internal feas, where a number of rivers bring an enormous quantity of water from all parts, without even increasing this immense lake, which seems to return return by subterraneous passages, all which it test ceives by its borders, and forming a passage, I easily discover the country anciently inhabited, I distinguish them from their new countries, where the soil appears rude, where the rivers are filled with cataracts, and where the land is partly overslowed, marshy or parched up, where the distribution of the waters is irregular, and where the uncultivated woods cover all the surface of the earth, which might pro-

duce vegetation.

Entering into a greater view, I find that the uppermost strata which surrounds the globe is every where the fame. That this substance which serves for the growth and nourishment of vegetables and animals, is itself only a composition of destroyed animals, and vegetable parts, in which the ancient organization is not perceptible. Penetrating still farther, I find the true earth, I fee beds of fand, stone, argol, shells, marble, gravel, chalk, &c. I remarked that thefe beds are always placed parallel to each other, and that each is of the same thickness throughout its whole extent. I fee that in neighbouring hills, the fame matters are alike even, although feperated by deep and confiderable intervals. I observe in all the beds of the earth, and even in the most solid strata, as rocks, quarries of marble, stone, &c. there are cracks, which are perpendicular to the horizon, and that in the largest, as well as in the smallest depths, it is a rule which nature constantly pursues. I fee besides in the internal part of the earth, on the ridges of mountains, and in the most remote parts of the sea, shells, skeletons of fishes, marine plants, &c. which are entirely fimilar to the shells, fishes, and actual living plants in the fea. I remark that there petrified shells are in a prodigious quantity, in an infinity of places, inclosed in the internal parts of rocks, and all other masses of marble and hard stone, as well as in chalk and earth; that as they

are not only shut up in these matters, but that they are incorporated with them, petrissed and filled with the same substance that surrounds them; at length, I find myself convinced, by reiterated observations, that marbles, stones, chalks, marl, sand, and almost all terrestrial substances are filled, with shells and all other matters of the sea throughout the earth, and in every place where exact observations have been made.

All this deposed, let us reason thereon.

The alterations which have happened to the terrestial globe, in 2 of 3000 years, are but very inconfiderable, in comparison of the revolutions which must have been made in the earliest time of creation: for it is easy to demonstrate, that all terrestial matters have acquired folidity, only from the continued action of gravity and other forces, which approach and unite together the particles of matter: the furface of the earth must have been at the beginning, much less folid than it afterwards was, and that confequently, the same causes which to day produces only almost insensible changes, in the space of many ages, must cause very great revolutions in fact, it appears certain, that the earth actually dry and inhabited, has formerly been under water, and that the water was higher than the tops of the mountains, fince we meet with many productions, which compared to the living shell fish are found to be the fame, and that we cannot therefore doubt of their perfect resemblance, nor the identity of their kinds; it appears also, that the water remained sometime on this earth, fince in every place we meet with fuch prodigious banks of shells, that it is not possible fo great a number of animals lived at one time: this feems also to prove, that although the matters which compose the surface of the earth, were in a state of softness, which rendered them easily to be divided, moved, and transported by the waters, there mo-VOL. V. tions tions were not made at once, but successively and by degrees: And as we fometimes meet with fea productions, at 1000 and 1200 feet depth, it appears that this thickness of the earth or stone being so confiderable, it required many years to produce it; for when we would suppose that in the universal deluge all the shell fish were raised from the bottom of the fea and transported over all the parts of the earth, that although this supposition would be difficult to establish, it is evident, that as we find those shells incorporated and petrified in marble and the rocks of the highest mountains, we ought therefore to suppose that those shells and rocks were all formed at one time, and precifely in the instant of the deluge; and that having this great revolution there were neither mountains, marble, nor rocks, nor chalk, nor any other matter fimilar to those we are at present acquainted with, which almost all contain shells and other ruinated productions of the fea. Besides, the surface of the earth, must, at the time of the deluge, have acquired a confiderable degree of folidity, fince gravity had acted on the matters which composed it, for more than fixteen centuries, and consequently it does not appear possible that the waters of the deluge were able to overturn the earth at the furface of the globe to fuch great depths in so little time as the universal inundation lasted.

But without dwelling on this point, which will be hereafter discussed I shall confine myself to observations which are constant, and to sacts which are certain. We cannot doubt, but that the waters remained on the surface of the earth we inhabit, and that consequently this surface of our continent has for some time been the bottom of the sea, in which every thing passed as it actually passes in the sea at present; besides, the strata of the different matters which compose the earth, being as we have remarked, placed parallel and even, it is evident

hat this position is the work of the waters, which have collected and accumulated by degrees those matters, and have given them the same fituation as the water itself takes. i. e. that horizontal fituation which we almost every where observe; for in plains the strata are exactly horizontal, and it is only in mountains where they are inclined, from their being formed by the fediment deposited on an inclined base. Now, I fay, that these strata were formed by degrees and not all at once by any revolution whatever because we often find strata of heavier matter placed on the strata of a much lighter, which could not be, if, as some authors will have it, all these matters deposited and mixed at the same time in the water, were afterwards precipitated to the bottom of this element: because, then they would have produced quite another composition, than that which exists. The heaviest matters would have descended the first and the lowest, and each would be arranged according to its specific gravity, in an order relative to its particular weight, and we should not find massive rocks on light sand, no more than coal, argol, clay, marble, and metals upon fand.

One thing to which we should pay attention, and which confirms what we have just faid on the formation of the strata by the motions and sediment of the water, is, that all the causes of revolution or change on the globe cannot produce the like effects, the highest mountains are composed of parallel strata, as well as the lowest plains, and consequently we cannot attribute the origin and formation of mountains to earthquakes, &c. no more than to Volcano's, and we have proofs that if small eminences are sometimes formed by these convulsive motions of the earth, they are not composed of parallel strata, that the matter of these strata have internally no bond, no regular position, and that these small hills formed by Volcano's, present to the fight only the disorder of a heap

a heap of matter thrown confusedly together; but this kind of organization of the earth which we every where discover, this horizontal and parallel fituation of the strata, cannot proceed but from a constant cause and a motion always regulated and directed in

the fame manner.

We are therefore affured by exact observations, reiterated and founded on incontestable facts, that the dryest part of the globe which we inhabit, has been a long time under water. Consequently this earth endured all that time, the same motions, changes, &c. which the earth now covered by the fea actually undergoes. It appears, that our earth was once the bottom of the sea; to find therefore what formerly passed on this earth, let us see what at present passes at the bottom of the sea, and from thence we shall derive reasonable inductions on the external form and the internal composition of the earth we inhabit.

Let us therefore remember, that the sea had, from the creation, a motion of flux and reflux caused principally by the moon; that this motion which twice in twenty-four hours raises and falls the water, is performed with greater power under the equator than in any other climate; let us also remember, that the earth has a rapid motion round its axis, and confequently a greater centrifugal force towards the equator than in all the other parts of the globe. That this alone independent of actual observations and measurements proves to us, that it is not perfectly spherical, but that it is more elevated under the equator, than on the poles, and let us conclude from these first obfervations, that whenever it is supposed that the earth came from the Creator's hands perfectly round, (a free supposition which marks the narrow circle of our ideas) its diurnal motion and that of the flux and reflux would have by degrees raifed the parts of the equator, by fucceffively bringing there mud, earth, thells, thells, &c. &c. thus the greatest irregularities of the globe must be found, and are, in fact, found near the equator, and as this motion of flux and reflux is made by diurnal alternatives, and repeated without interruption, it is very natural to imagine each time the water carries a small quantity of matter from one part to another, which afterwards falls like a sediment to the bottom of the water, and forms these parallel and horizontal strata every where to be met with; for the whole motion of the water in the flux and reflux being horizontal, the matters carried away with it have necessarily followed the same direction, and are all arranged parallel and even.

But it may be faid, as the motion of flux and reflux is an equal counterpoise of the water, a kind of regular oscillation, we cannot see why all should not be compensated, and the matters brought away by the flux, should not be returned by the reflux, and from thence the cause of the formation of the strata would disappear, and the bottom of the sea would always remain the same, the flux destroying the effects of the reflux, both theoneand the other therefore would not be able to cause any motion or sensible alteration in the bottom of the sea, or still less change the primitive form of it by producing height and inequalities therein.

To this I answer, that the counterpoise of the water is not equal, since it produces a continual motion of the sea from the east to the west; that besides the agitations caused by the winds, opposes the equality of the flux and the reslux, and from all the motions of which the sea is susceptible, there will always result transportations of earth and deposits of matters in certain places; that these masses of matters will be composed of parallel and horizontal strata, the various combinations of the motions of the sea always tending to move the earth and to level it where

where it falls in form of a fediment; but likewife, it is eafy to answer this observation by one fact, which is, that wherever the flux and reflux is observed, in all the coasts which border the sea, we find that the flux brings an infinity of things which the reflux does not carry back, that there are soils which the sea insensibly covers, and others which it wears bare, after having brought thither earth, sand, shells, &c. which it deposits, and which naturally takes an horizontal situation, and that the matters accumulated by the course of time and raised to a certain point, find themselves out of the reach of the water, afterwards remain in a state of dryness, and make a part of the terrestrial continents.

But not to have any doubt on this important point, let us narrowly examine the possibility or impossibility of the formation of a mountain in the bottom of the fea, by the motion and fediment of the waters. No one can deny that on a coast where the fea beats with violence in the time it is agitated by the flux, its reiterated efforts produce no change, and that the water does not carry away a fmall portion of the earth each time, and wherever it is bounded by rocks, we know that water by degrees wears away these rocks, and consequently carries away fmall parts each time the wave retires, thefe particles of earth and stone will necessarily be transported by the water to a certain diffance, and into places where the motion of the water being abated, and these particles left to their own weight, they will precipitate to the bottom of the water in form of a fediment, and there form a first horizontal or inclined strata, according to the position of the surface of the foil on which they fall, which will foon be covered with another fimilar strata produced by the like cause, and insensibly these will form in this part a confiderable deposit of matter, the strata of which will will be placed parallel to each other; this firata will increase by the new sediments which the water will transport thither, and by degrees in succession of time, thefe will form an elevation or mountain at the bottom of the fea, which will be fimilar to the eminences and mountains we are now acquainted with on the earth, as well with respect to the internal composition as the external form. If shells are found in this part of the fea where we suppose our deposit to be made, the fediments will cover them and fill them, they will be incorporated in the strata of this deposited matter, and will make part of the masses formed by their deposits, we shall there find them in the fame fituation they acquired in falling, for in this operation those which will be found at the bottom of the fea when the first strata shall be deposited, will be found in the lowest, and those which will be fallen in this fame fpot will be found in the more elevated ftrata.

So likewife, when the bottom of the fea is moved by the agitation of the waters, there will necessarily enfue transportations of shells, mud, earth, and other expert matters into certain places where they will deposit in form of fediment. Now, we are affured by Divers, that at the greatest depths to which they can descend, i. e. 20 fathom, the bottom of the fea is moved fo much that the water mixes with the earth, becomes troubled, and that the mud and shell fish are carried by the motion of the waters to confiderable diffances, confequently in every part of the fea, to which we can dive, transportations of earth and shells are made, fome part of which falls and forms parallel strata and eminences, which are composed like our mountains; therefore the flux and reflux, the wind, the currents and all the motions of the waters, will produce inequalities at the bottom of the fea, because all these causes loosen from the bottom or sides of the sea, matters, which afterwards precipitate in form of fediment.

On the whole, we must not think that these traisportations of matters cannot be made to great distances, since we every day see grains and other productions of the East and West Indies arrive on our coasts, particularly on those of Scotland and Ireland; in fact, they are specifically lighter than the water, whereas the matters of which we speak are heavier; but as they are reduced into an impalpable power, they will sustain a long time in the water,

fo as to be transported to great distances.

Those who pretend that the sea is not stirred at great depths, do not consider that the flux and re-flux shake and agitate at once the whole mass of the sea, and that in a globe entirely liquid, there will be agitation and motion to the very center. That the force which produces the flux and re-flux, is a penetrating force, which acts on every part proportionably to its mass; that we can even measure and determine by calculation the quantity of this action on a liquid at different depths; and, that in short, this point cannot be contested but by refusing the evidence of

reason, and the certainty of observations.

I therefore can lawfully suppose, that the flux and re-flux, the wind and every other cause which can agitate the sea, must by the motion of the waters produce eminences and inequalities at the bottom of the fea, which will be always composed of horizontal or Thefe eminences with time, equally inclined strata. may augment confiderably and become hills, which in a long extent of foil, will be found like the waves which produced them in the fame direction, and by degrees will form a chain of mountains. These heights once formed, will form an obstacle to the motion of the water, and there will refult particular motions in the general motion of the fea. Between two neighbouring heights, a current will be necessarily formed, which will follow their common direction, and will flow as the rivers of the earth flow, by forming a canal whose angles will be alternatively opposite in the whole extent of its courfe. These heights formed above the furface of the deep, will fill more and more increase, for the water, which will only have the motion of the flux, will deposit on the ridge the. common fediment, and those which will obey the current, will drag along with them, at a distance. the parts which would be deposited between both; and. at the fame time, they will hollow a valley at the foot of these mountains, whose angles will be correspondent, and by the effect of these two motions and deposits, the bottom of the sea will soon be found traverfed with hills and chains of mountains, and scattered with inequalities, fuch as we at prefent meet with. By degrees the foft matters of which the eminences were at first composed, will be hardened by their own weight; fome forming parts purely angular, will produce hills of clay which we find in many parts; others composed of fandy and christalline parts, have made these enormous masses of rocks and flints, from which we extract the christal and other precious stones: others formed of stony parts mixed with shells, have formed those beds of stone and marble. where we every day meet with shells, &c. and others compounded of a matter still more shelly and terrestrial, have produced marl, chalk and earths. All are placed by beds, all contain heterogeneous fubstances; the wrecks of marine productions are found there in abundance, and nearly according to the relation of their weight; the lightest shells are in chalk, the heavieft in argol and stone, and they are filled even with the matter of the stones and earths wherein they have been shut up. An incontestible proof that they have been transported with the matter which furrounds and fills them, and that this matter was reduced into impalpable particles: in short, all these matters whose fituation was established by the level of the waters of the sea, will still preserve their first position. Vol. V. Tt It

It might be faid, that most hills and mountains, whose fummits are rock, stones or marble, have the lightest matter for their base, which are generally either little mountains of firm and folid clay, or strata of fand, which we find in the neighbouring plains; and it will be asked, how it happens that these marbles and rocks are found above the fands and clays. This appears to me eafily and very naturally explain-The water at first transported the clay or fand, which formed the first strata at the bottom of the fea, which produced at bottom, an eminence composed of all this fand or clay collected together. After that the more firm and heavy matters, which are found above, will be attached and tranfported by the water in an impalpable powder above this eminence of clay or fand, and this stony powder will have formed rocks and quarries which we find on the tops of hills. It might be thought that being the heaviest, these matters were formerly below the others, and that they are at prefent above, because they have been raifed up and transported by the motion of the water.

To confirm what we have faid, let us still more clearly examine the fituation of matters which compose this first thickness of the terrestrial globe, the only one which we are acquainted with. The quarries are composed of different beds of strata almost horizontal, or inclined according to the same direction; those which are deposited on clay, or on bases of other solid matters, are sensibly even, especially in planes. The quarries wherein we find flints and grés, have indeed a less regular position, nevertheless the uniformity of nature does not fail of difcovering itself; for the terrestrial position either alwavs equally inclined from the strata, is found in quarries of stone and in those of gres in great masses; it is only interrupted and changed in quarries of flint and gres in small masses, the formation of which

we shall shew is posterior to that of all other matters; for stone, vitrifiable fand, argol, marble. calcinable stone, chalk and marl, are all disposed in parallel strata, always horizontally or equally inclined. We eafily discover the first formation in these matters, for the strat are exactly horizontal and very thin, and are arranged as are the other, like the leaves of a book; the strata of fand, foft argol. hard clay, chalk and shells, are also all either horizontal or inclined according to the same direction, the thickness of the strata are always alike throughout all their extent, which often occupies a space of many miles, and which we might follow much farther, if we exactly observed it. In short, all matters which compose the first thickness of the globe, are disposed after this manner, and whatever part we strip off, we shall find strata, and be convinced by

our eyes of what we have just advanced.

In some respects we must except the strata of fand and gravel carried along the tops of the mountains by the inclination of the waters. These veins of fand are found fometimes in plains wherethey extend very confiderably, they are generally placed under the first strata of earth, and in flat places they are as even as the oldest and most inward strata; but, at the foot of mountains these strata of fand are very inclined, and follow the direction of the height from which they have flowed. Rivers and rivulets have formed thefe strata, and by often changing their bed in plains, they have dragged with them, and every where deposited these sands and gravel. A small rivulet flowing from neighbouring heights, fuffices with time to extend a strata of fand or gravel over all the superfluities of a valley however spacious it is, and I have often observed in countries surrounded with hills. whose base is clay as well as the first strata of the plain, as above a rivulet which flows there, the clay is found immediately under the earth; and below the rivulet

rivulet there is the thickness of a foot of fand on the clay which extends a confiderable way. These strat produced by the rivers and other running waters, are not of ancient formation, they are eafily perceptible by the difference of this thickness, which varies and is not every where the same like those of the ancient strata, by these frequent interruptions, and in short, by the matter itself, which is easily to be discovered to have been washed and rounded. We may say the same of the strata of turf and perished vegetables which are found below the first strata of earth in marshy lands: those strat are not ancient and produced by the fuccessive heaping of trees and plants, which by degrees have filled these marshes, it is the fame with those muddy strata which the inundation of floods have produced in different countries; all these soils have been newly formed by running or stagnant waters, and they do not follow the equal inclination, or the evenness to earth as the strata anciently produced by the regular motion of the waves of the sea. In the strata formed by the rivers, we find fluviatile shells, but there are few marine, and what few are found, are broken, displaced, and is olated; whereas the marine shells are formed in great quantities in the ancient strata; there are no fluviatile shells there, and the sea shells are well preferved and all placed in a like manner, as having been transported and deposited at the same time and by the same cause: in fact, why do we not meet with matter irregularly heaped up instead of finding them in strata? why are not marble, stone, chalk, marl, &c. dispersed or joined by irregular or vertical ftrata? why are not the heaviest things below the lightest? It is easy to perceive that this uniformity of nature, this organization of the earth, this junction of different matters by parallel strata and beds, without respect to this weight, have not been produced but by a cause as powerful and as constant as the agitation agitation of the water, whether by the regular motion of the winds, or by that of the flux and reflux, &c.

These causes act with greater power under the equator than in other climates for the winds are the more constant, and the tides more violent than elsewhere; the greatest chain of mountains also, are near the equator; the mountains of Africa and Peru, are the highest known, and after having traversed whole continents, they still extend to very confiderable distances under the ocean. The mountains of Europe and Afia, which extend from Spain to China, are not so high, as those of South America and Africa. The mountains of the North are, by the relation of travellers, only hills in comparison of those of the southern countries; besides, the number of islands is very inconsiderable in the northern feas, whereas there is a prodigious quantity in the torrid zone, and as an island is only the top of a mountains, it is clear that the furface of the earth has many more inequalities towards the equator, than towards the north.

The general motion therefore of the flux and reflux, has produced the greatest mountains, which are found directed from the west to the east in the old continent, and from the north to fouth in the new, the chains of which are of a very confiderable extent; but, we must attribute to the particular motion of currents, winds, and other irregular agitations of the sea, the origin of all other mountains; they have probably been produced by the combination of all these motions, of which we see the effects must be varied, ad infinitum, fince the wind, the different position of islands and coasts, have at alltimes changed, in all possible directions the flux and reflux: therefore, it is not at all aftonishing, that we find confiderable eminences on the globe, whose courses are directed towards different points.

It is sufficient for our purpose, to have demonstrated that mountains have not been placed at random, nor produced by earthquakes, or other accidental causes; but, that they are an effect resulting from the general order of nature, as well as a kind of organization which is proper to them and the position of

matters which compose it.

But how has it happened that this earth which we inhabit, and our ancestors have inhabited before us, which has been a dry continent for time immemorial, shut up and remote from the sea, having formerly been the bottom of a sea, is actually larger than all the water, and is so distinctly separated from it? Why did not the water remain on this earth, since it stayed there so long? What accident, what cause has been able to produce this change in the globe? Is it possible to conceive one powerful enough to perform such an effect?

These questions are difficult to be resolved; but the facts being certain, the manner in which they have happened may remain unknown without doing any prejudice to the judgment we form; nevertheles, if we reflect thereon, we shall find by induction very plaufible reasons for these changes. We daily fee the fea gain some land on certain coasts, and lose it on others, we know that the ocean has a general continued motion from east to west, we hear from far, the terrible efforts which the fea makes against the lowlands and against the rocks which confine it; we know of whole provinces which are obliged to oppose dykes which the industry of men can hardly support against the rage of the sea; we have recent examples of countries funk under water. History speaks farther of still greater inundations and deluges: must not all this incline us to believe, that in fact great revolutions have happened on the furface of the earth, and that the fea has gone from and left naked the greatest part of the earth it formerly

covered? If we a moment give way to suppose the old and new world formerly made but one continent, and that by a violent earthquake the land of the old Atlantic of Plato is swallowed up, the sea will neceffarily have flowed from all fides to form the Atlantic ocean, and confequently will have left dry vaft. continents, which are possibly these we inhabit. This change therefore might be made all at once by the opening of some vast cavern in the internal part of the globe, and possibly produce an universal deluge: or possibly this change was not made all at once, and it perhaps required fome time, but, at length it was done, and I even think very naturally fo, for to judge of what has happened, as likewife of what will happen, we have only to examine what does happen. It is certain, by the reiterated observations of travellers, that the ocean has a constant motion from east to west: this motion is felt not only between the tropics, as the east wind, but also throughout the extent of the temperate and frigid zones: from this observation, it follows, that the Pacific ocean makes a continual effort against the coasts of Tartary, China and India; that the Indian ocean makes an effort against the oriental coast of Africa, and that the atlantic ocean acts in like manner against all the eastern coasts of America. Therefore the sea ought and must always gain land on the eaftern coafts and lose it on the western. This alone fuffices to prove the possibility of this change of earth into fea and fea into earth, and if in fact, it is performed by this motion from east to west, as there is a great appearance, can we not very probably conjecture that the oldest country in the world is Asia and all the eastern continent? That on the contrary Europe and a part of Africa, and especially the western coasts of these continents, as England, France, Spain, Muritania, &c. are more modern lands? History appears appears here to agree with Phyrie, and to confirm this

conjecture which is not without foundation.

But there are many other causes which concur with the continual motion of the fea from east to west, to produce the effect of which we speak. How many countries are there not lower than the level of the fea. and which are defended from it only by an ifthmus; a bank of rocks, or by ftill weaker dykes? The efforts of the waters will by degrees deftroy these barriers, and then these countries will be overflown. Befides, do we not know, that mountains continually lower by the rains which loofen the earth and wash it into the valleys? Do we not know that floods wash the earth and fand from the plains and mountains into the rivers, which in their turn carry this superfluous earth into the fea? Thus by degrees the bottom of the fea is filled up, the furface of the continent lowered and levelled, and time is only required for the fea

fuccessively to take place of the earth.

I fpeak not of these remote causes which we foresee less than we foretell, of these shocks of nature, the least effect of which would be the catastrophe of the world, the shock or approach of a comet, the absence of the moon, the presence of a new planet, &c. are fuppositions on which it is easy to give scope to imagination; fuch causes would produce every thing we would speak of, and from one of these hypothesis's, others and physical romances may be drawn, which their authors will call the Theory of the Earth. historians we reject such vain speculations they term impossibilities, which, suppose the destruction of the universe, in which our globe, as a point of forsaken matter, escapes our fight and is no longer an object worthy our regard; to fee them, we must take it such as it is, by well observing every part, and by inductions conclude the past from the prefent, in other respects the causes whose effect is so rare violent and fudden, must not effect us, they are not to be met with with in the common course of nature; but effects which every day happen, motions which succeed and renew themselves without interruption, and constant and reiterated operations, are our causes and reasons.

Let us add examples thereto, let us combine the general cause with particular causes, and give facts; the detail of which will render apparent the different changes which have happened on the globe, either by the irruption of the ocean into the land, or by for-

faking it when it became too high;

The greatest irruption of the ocean was that which produced the Mediterranean fea. Between two promontories the ocean flowed with great rapidity through a narrow channel which afterwards formed a vast sea; covering a space, which without including the black sea, is about seven times larger than France. This motion of the ocean through the straits of Gibraltar is contrary to all the other motions of the sea in all the straits which join ocean to ocean, for the general motion of the fea is from east to west, and that alone is from west to east, which proves that the mediterranean sea is not an ancient gulph of the ocean, but that it has been formed by an irruption of water produced by some accidental causes, as an earthquake, which might have swallowed up the earth in the strait, or a violent effort of the ocean caused by the wind, which might have broken the dykes between the promontaries of Gibraltar and Ceuta. This opinion is grounded on the teftimony of the ancients, who have written that the mediterranean sea did not formerly exist, and it is as we fee, confirmed by natural history, and by observations made on the nature of the lands on the coast of Africa and that of Spain, where we meet with the same beds of stone, the same strata of earth above and below the strait, nearly as in certain valleys where the Uu

two hills which furmount them are found to be composed of the like matters and are of the same level.

The ocean having therefore opened this door, at first slowed through the strait with a much greater rapidity than at present, and overslowed the continent which joined Europe to Africa. The waters covered all the low countries, of which we at present only perceive the eminences and summits in Italy, and the islands of Sicily, Malta, Corsica, Sardinia,

Rhodes, and Archipelago.

I have not included the black fea in this irruption, because the quantity of water which it receives from the Danube, Nieper, Don, and many other rivers which enter therein, is more than fufficient to form it; and that befides, it flows with a very great rapidity through the Bosphorus into the mediterra-It might also be presumed that the Black and the Caspian sea formerly formed only two great lakes, which perhaps were joined by a strait of communication, or possibly by a morrass, or a small lake which united the Don and the Volga near Tria, where thefetwo floods are very close to each other; and it may be thought, that these two seas or lakes were formerly of a much greater extent than they are at present: by degrees these great rivers, whose mouth is in the Black and Caspian seas, will have brought a fufficient quantity of earth to shut up the communication, fill the strait and separate these two lakes; for we know that by time the greatest rivers fill up feas, and form new continents, as the province at the mouth of the Yellow river in China, Louisania at the mouth of the Mississipi, and the northern part of Egypt, which owes its origin and existence to the inundations of the Nile. The rapidity of this river carries along with it the earth of the internal part of Africa, and deposits it afterwards on its shore in such great quantities, that it may be found at the depth of 50 feet, deposited by the inundations of the Nile: so likewise the

the strait of the province of the Yellowriver and Lou-

isania are only formed by the soil of the rivers

On the whole the Caspian sea is actually a real lake which has no communication with the other feas, not even with the lake Aral which feems to have been a part of it, and which is only separated from it by a vast country of fand, in which we find neither floods, rivers, nor any canal by which the Caspian sea may water it. This therefore has no communication with the other feas, and I do not know whether we are not properly authorized to fuspect that it has internally one with the Black fea or Perfian gulph. It is true the Caspian sea receives the Volga and many other floods which feems to furnish it with more water than evaporation could raife; but, independent of the difficulty of this estimation, it appears, that if it had a communication with one or other of these seas, it would have been discovered there by a rapid and conflant current which would have fwept every thing along with it towards this opening, which ferve for the discharge of its waters; and I do not know whether any thing like this has ever been observed on that sea: careful travellers, on whose testimony we can rely, affirm the contrary, and confequently it is necessary that should evaporation raise up from the Caspian sea an equal quantity of water to that which it receives.

It may also be conjectured that the Black sea will be one day separated from the Mediterranean, and that the Bosphorus will be filled, when the great rivers whose mouths are in the Port-Euxine, shall have brought a sufficient quantity of earth to shut up the strait, which may happen in and by the successive diminution of water in proportion as the mountains and highlands from which they drew their source, lower by the devastation of the earth which the rains

and the wind fweep away.

The Caspian sea and the Black sea must therefore be looked upon rather as lakes than as seas or gulphs, for they resemble other lakes which receive a great number of rivers, and which render none back by external roads as the Dead sea, and many lakes in Africa, &c. In other respects, the two seas are not near so salt as the Mediterranean or the ocean: and all travellers affirm that navigation is very difficult on the Black sea and on the Caspian sea, by reason of their shallowness and the quantity of shoals and quicksands there met with, so that none but small vessels can pass, which still proves that they must not be looked upon as gulphs of the ocean, but as masses of water formed by great rivers in the internal parts of the land.

Perhaps a confiderable irruption of the ocean would happen to the earth, if the isthmus, which separates Africa from Afia, was divided as the Kings of Egypt, and afterwards the Caliphs projected, and I do not know whether the canal of communication pretended to be discovered between these two seas. is sufficiently established, for the Red sea must be higher than the Mediterranean; this narrow fea is an arm of the ocean which throughout all its extent does not receive any river from the coast of Egypt, and very little from the other coasts: it will not therefore be subject to diminish like the sea or lakes which receive at the same time the earth and water which the river brings there, and which by degrees fill it. The ocean supplies the red sea with its water, and the motion of flux and reflux is extremely fenfible there, therefore it directly participates of the movements of the ocean. But the Mediterranean is lower than the ocean, fince the water flows here with very great rapidity through the strait of Gibraltar; befides, it receives the Nile, which flows parallel to the western coast of the Red sea, which divides Egypt, the foil of which is extremely low: therefore

it is very probable that the Red sea is higher than the Mediterranean, and if we put aside the barrier by cutting the isthmus of Suez, a great inundation and a considerable augmentation of the Mediterranean will ensue, at least if the waters are not retained by dykes and sluices at stated distances, as it is to be presumed was formerly done, if the ancient canal of communi-

cation had existed.

But without dwelling longer on conjectures, which although founded, might appear too hazardous, efpecially to those who judge of possibilities only by actual events, we can give recent examples and certain facts of the change of the fea into earth, and earth into At Venice the bottom of the Adriatic fea raifes every day, and the whole would have made part of the continent, if great care had not been taken to clean and empty the coasts; it is the same with most parts, small bays, and inlets of all rivers. In Holland the bottom of the fea also rises in many places, for the little gulph of Zuyderzee and the strait of the Texel cannot receive fuch large veffels as formerly; at the mouth of all rivers we find islands, fands, earth broken and brought by the water, and it is not to be doubted that the fea would fill in every part where it receives great rivers. The Rhine is loft in the fands itself has accumulated. the Danube and the Nile, and all the great floods having fwept away much earth, no longer comes to the fea by a fingle canal, but has many mouths whose intervals are filled only with the fand or mud they have carried away; every day moraffes dry up, lands forfaken by the fea are cultivated, navigation is carried on even drowned countries; in fhort, we fee before our eye fuch great changes of earth into water, and water into land, to be affured that these alterations are made, are still made, and will be made, for that in time gulphs will become continents, ifthmus's will be one day ftraits, moraffes will be fome day lands, lands, and the tops of our mountains the shoals of the fea.

The waters therefore do cover and may yet cover fuccessively every part of the terrestrial continent; hence we must cease our surprise to find every where marine productions, and a supposition which can only be the work of the waters. We have observed, how the horizontal strata of the earth are formed, but we have as yet spoken nothing of the perpendicular divisions, remarked in rocks, quarries, &c. and which are as commonly found as strata in all matters which compose the globe. These perpendicular strata are in fact at a farther distance one from the other than the horizontal strata, and the foster the matters are, the more distant these cracks are from each other; it is very common in quarries of marble or hard stone, to find perpendicular cracks, distant from each other only a few feet; if the mass of rocks are very great, we find them fome fathom distant, fometimes they deicend from the top of the rock to the bottom; often terminate in a lower bed of rock, but they are always perpendicular to the horizontal frata in all calcinable matters, as chalk, stone, marl, marble, &c. whereas they are more oblique and irregularly placed in vitrifiable matters, in quaries of gres and flint, where they are internally adorned with chrystal points, and minerals of all kinds, and in quarries of marble and calcinable stone, they are filled with spar, gypsum gravel, and an earthy fand, good for the purpose of building, and which contains much lime; in clay chalk, marl, and every other kind of earth excepting turf, we find thefe perpendicular cracks, either empty or filled with fome matters which the waters has conducted thither.

It appears, that we must not seek farther for the cause and origin of these perpendicular cracks; as all matters have been carried and deposited by the waters, it is natural to think that they were moistened

and at first contained a great quantity of water, by degrees they hardened, and in drying leffened their volume, which caused them to split; they must have fplit perpendicularly, because the action of the weight of the parts one on the other is not in this direction, and on the contrary perfectly opposed to this disruption in an horizontal fituation, which has caused the diminution of the volumes to have any sensible effect but in a vertical direction. I say, that it is the diminution of volume by drying, which alone has produced these perpendicular cracks, and not the water contained in the internal part of these matters, which feeking for a place to iffue out at, caused these cracks: for I have often observed that the two sides of those cracks answer throughout their whole height as exactly as two split pieces of wood, their inside is rough and does not appear to have endured the friction of the water, which would have polished the furface; therefore, these cracks are either made all at once, or by degrees in drying, as we fee flaws in wood are made, and the greatest part of the water is evaporated through the pores. The division of these perpendicular cracks raises greatly with respect to fize, fome being only half an inch, and others a foot or two feet, there are some which often are many fathom, and there form between the two parts of the rock those precipices so often met with in the Alps and other high mountains; those which are fmall are produced by the drying alone, but those which prefent an opening feveral feet broad are not increased to that point, by this cause alone: it is also occasioned by the base which bears the rock, which is funka little more on one fide than the other, and a small finking in the base; for example, of a line or two is fufficient to produce openings many feet wide in a confiderable height: fometimes also rocks give a little clay or fand on their base, and the perpendicular cracks become larger by this motion. I do not yet

yet speak of those large divisions formed in rocks and mountains; they were produced by great finkings; like as that of a cavern would be which not being able longer to support the weight with which it is loaded, gave way and left a confiderable interval between the upper earth: those intervals are different from perpendicular cracks, they appear to be vacancies opened by the hands of nature for the communication of nations. It is in this manner, which the vacancies present themselves in the chains of mountains and divisions of the straits of the sea, as the Thermopyles, the ports of Caucasus, Cordiliers, and the extremity of the strait of Gibraltar, between the mountains Calpa and Abyla, the entrance of the Hellespont, &c. those vacancies have not been found by the fimple separation of matters, as the cracks we speak of, but by the finking and destruction of a part of the lands themselves which have been swallowed

up or overturned.

These great finkings, although produced by accidental causes, do, notwithstanding, hold one of the first places in the principal circumstances of the history of the earth, and they have not a little contributed to change the face of the globe: the greatest numbers are caused by internal fires, whose explosions cause earthquakes and volcano's; nothing is comparable to the force of those inflamed and confined matters in the bowels of the earth; we have feen cities fwallowed up, provinces overturned, mountains overthrown by their effort. But however great their violence is, however prodigious the effects appear, we must not think that this fire proceeds from a central fire, as some authors have written, nor even that they proceed from a great depth, as is the common opinion; for air is absolutely necessary for their fupport: it may be afferted upon examining the matters which iffue from volcano's in the most violent irruptions, that the forces of the inflamed matters is not at any great depth, and that they are fimilar matters to thole found on the earth of mountains, which are disfigured only by the calcination and melting of the metallic parts mixed therewith; and to convince, that these matters cast out by the volcanos do not come from any great depth, we have only to confider the height of the mountain, and judge of the immense force that would be necessary to force out stones and minerals to the height of half a mile; for Ætna, Hecta, and many other volcanos are at least that height from the plains. Now it is known that the action of fire is made in every direction; it cannot therefore be exercifed upwards with a force capable of throwing out large stones to the height of half a mile, without reacting with the same force downwards, and towards the fides; this re-action would have foon destroyed and pierced the mountain on every fide, because the matters which compose it are not harder than those which arethrownout; and, how is it to be imagined that the cavity which ferves for barrel or cannon to conduct those matters to the mouth of the volcanos, could refift such great violence? Besides, if this cavity defcended fo low, as the external orifice is not very great, it would be as impossible for so great a quantity of inflamed and liquid matter to iffue out at once, because they would strike against each other and against the fides of the tube, and by going thro' so great a space, they would be extinguished and hardened. From the summit of the mountain we often see flow into the plains, rivers of bitumen, and melted fulphur, which comes from within, and which are thrown out with the stones and minerals: it is natural to imagine that matters fo little folid, and whose mass gives such little force to a violent action, could be thrown out from fo great a depth: all the observations which may be made on this subject will prove that this volcanian fire is not far dif-Xx

tant from the fummit of the mountain, and that it

even does not descend to a level with the plain.

This however, does not prevent its action from being felt in those plains by shocks and earthquakes which sometimes extend to very great distances; or, that there may not be fubterraneous ways through which the flame and fmoke may communicate with another volcano, and that in this case they could not act and inflame almost at the same time, but it is from the focus of the confinement of which we fpeak, it can only be at a small distance from the mouth of the volcano, and it is not necessary to produce an earthquake in the plain, that this focus be below the level of the plain, nor that there is internal caverns filled with the same fire; for a violent explofion, fuch as that of a volcano, can, like that of a powder magazine, give fo violent a shock, that by its re-action it may produce an earthquake.

I do not pretend to fay, that there are earthquakes immediately produced by subterraneous fires; but,

that there are some which proceed only from the explosion of volcanos alone. What confirmes all I have advanced on this subject is, that it is very rare to meet with volcanos in plains; on the contrary, they are all in the highest mountains, and have all their mouths at the summit: if the internal fire which consumes them extends below the plains, should not we see in the time of these eruptions a passage open across the plains, and during the first eruption, these fires, would they not rather have pierced the plains and the foot of mountains where they would have met with a weak resistance, in comparison of that which they ought to meet with, if it is true that they have opened and split a mountain half a mile high

to find vent.

What causes the volcanos to be always in the mountains, is that the minerals, pyrites, are found in a greater quantity and more exposed in mountains

What

than in plains, and that these losty places receiving more readily, and in a greater quantity the rain and other impressions of the air; these mineral matters exposed there, ferment and heat to the point of be-

ing inflamed.

In short, it has often been observed, that after violent eruptions, during which the volcano threw out a very great quantity of matter; the summit of the mountain sunk and diminished to supply the matters thrown out: another proof, that they do not proceed from the internal depth of the foot of the mountain, but from the nearest part of the summit, and from the summit itself.

Earthquakes have therefore, in many points produced confiderable finkings, and have caused some of the greatest separations found in the chain of mountains: all the rest have been produced at the fame time, as the mountains themselves by the current of the fea, and in every place where there has not been an overthrow, we find horizontal strata and corresponding angles of mountains. Volcanos have also formed caverns and subterraneous excavations, which are easy to be distinguished from those formed by the water; which having washed away from the infide of mountains, the fand and other divided matters, have left only the stones and rocks which contained these fands and have thus formed caverns which we observe in high places; forthose found in the plains are commonly only ancient quarries or mines of falt and other minerals, as the quarry of Mastricht and the mines of Poland, &c. which are in plains; but natural caverns belong to mountains, and they rereceive the water from the fummit and environs, which fall therein as in refervoirs, from whence they afterwards flow over the furface of the earth when they find vent. It is to these cavities that we must attribute the origin of springs, and when a cavern is filled, an inundation generally enfues.

What we have observed is every where to be seen. how greatly the fubterraneous fires contribute to change the furface and internal part of the globe: this cause is sufficiently powerful to produce such great effects; but it would not be believed that the world could cause any sensible alterations on the earth; the fea appears to be their empire, and after the flux and reflux, nothing acts with greater power in this element: even the flux and reflux moves in a uniform path, and their effects are performed in an equal manner, and which we foresee; but if the impetuous winds, if I may fo express myself, by caprice, precipitate themselves with rage, and agitate the sea with such violence, that in an instant this calm and tranquil plain becomes furrowed with waves as high as mountains, which break against the rocks and coasts. The winds change therefore every moment, the moveable face of the sea; but the face of the earth which appears to us fo folid, is not subject to a like effect? We know, nevertheless, the wind raises lift-up mountains of fand in Arabia and Africa, that they cover plains with it, and that they often transport fands to great distances and many miles into the sea, where they collect in such great quantities, as to form banks, downs, and islands. known that the hurricanes are the scourge of the Antiles, Madagascar, and other countries, where they act with fuch fury that they fometimes tear up trees, plants, and carry off animals with all the cultivated land; they cause rivers to ascend and descend; they overthrow mountains and rocks, they make holes and gulphs in the earth, and entirely change the furface of the unfortunate countries where they are found.

Happily there are but few climates exposed to the impetuous fury of these terrible agitations of the

air.

Bua what produces the greatest and most general changes in the surface of the earth, are the waters of the

the heavens, floods, rivers and torrents. Their first origin proceeds from the vapours, which the fun raises above the surface of the sea, and which the wind transport into every climate. These vapours fustained in the air and impelled at the will of the wind, fix on the fummits of mountains they encounter in their way, and those accumulate in such great quantities, that they continually form the clouds, and incessantly fall in form of rain, dew. fnow, &c. All these waters at first descend upon the plains without keeping any fixed course, but by degrees, having hollowed their bed, and feeking by their natural bent the lowest parts of the mountains, and the land the easiest to divide or penetrate; they have dragged away earth and fand, they have formed rivers, by flowing with rapidity along the plains; they have spread passages to the sea, which receives as much water by its shores as it loses by evaporafloods have hollowed, have finuofities, &c. whose angles are correspondent to each other, so that tion; and fo likewife, the canals and ruts which the one of these waves forming a saillant angle in the land, the opposite shore makes always an opposite The mountains and hills, which must be looked upon as the borders of the vallies which seperate them, have also corresponding finuofities after the fame manner, which feems to demonstrate that the vallies have been once currents of the feas which have by degrees hollowed them in the fame manner as the floods have hollowed their bed in the earth.

The waters on the furface of the earth, which supports its verdure and fertility, are perhaps only the smallest part of those which the vapours produce: for there are veins of water which slow, and of moisture which filter to great depths in the internal part of the earth. In certain places, in whatever part we dig, we are sure to meet with springs in others, we find none at all. In almost all vallies and

low plains we feldom fail of finding water at a moderate depth: on the contrary, in all high places, and in all the plains in a mountain, we cannot draw it from the bowels of the earth, and are obliged to collect the waters of the heaven. There are countries of a vast extent, where a spring has never been able to be dug, and where all the water which ferve the inhabitants and animals, are contained in marshes and cifterns. In the east, and especially in Arabia, Egypt, Persia, &c. the pits are extremely scarce, as well as the springs of fresh water, and those people are obliged to make a great refervoirs to collect the rain and fnow water. These works made for the public necessity, are perhaps the most beautiful and magnificent monuments of the eastern nations : there are refervoirs which are two miles in furface, and which ferve a whole province, by means of baths and fmall rivulets on every fide. In other countries, on the contrary, as in the plains where the greatest floods of the earth flow; we cannot dig far from the furface without meeting with water, and in a field fituate on the environs of a river, it is met with at a few strokes of a pick-ax.

This quantity of water found in every part of low places, comes from the higher lands and neighbouring hills, at least, the greatest part of it from rainy and fnowy weather; a part of the waters flow on the furface of the earth, and the rest penetrates it through the fmall cracks of earth and rock: and this water fprings up in different places where it finds vent, or else filters into the fand, and where it finds a clayey, folid, or earthy bottom, it forms lakes, rivulets and perhaps fubterraneous floods, whose course and mouth are unknown to us, but the motion of which, nevertheless by the laws of nature, cannot be made but by going from a higher place into a lower, and consequently these subterraneous waters must fall into the sea, where it collects in some low place of the earth,

earth, either on the furface, or in the internal part of the globe; for we know fome lakes on the earth. in which there does not enter, and from which there does not iffue any river, and there is a much greater number, which not receiving any confiderable river on the fources of the greatest floods of the earth, as the lakes of St. Laurence, the lake Chiam, from whence fpring two great rivers which water the kingdoms of Aram and Pega, the lakes of Affiniboils in America, those of Ozera in Muscovy, that from which the great river Irtis issues, &c. and an infinity of others which feem to be the refervoirs from whence nature feems to pour from all fides, the waters fhe diffributes over the furface of the earth. We see then perfectly, that these lakes cannot be produced but by the waters of the upper lands, which flow by most fubterraneous canals by filtering the gravel and fand, and all proceed to collect in the lowest places, where great maffes of water are found. On the whole, it must not be thought, as some have advanced, that lakes are to be found on the fummit of the highest mountains; for those found in the Alps and in all other lofty places, are always furrounded by much higher lands, and are at the foot of other mountains perhaps more lofty than the first; they derive their origin from the water which flows on the outfide, or filters within these mountains, the same as the waters of the valleys and plains derive their fources from the neighbouring hills and the more distant lands which o'ertop them.

There must therefore be, and in fact is found, lakes and waters dispersed in the bowels of the earth, especially under plains and great vallies, and mountains, hills and all eminences, present either a perpendicular or inclined inclination, in the extent of which the waters which fall on the summit of the mountains and high plains, after having penetrated

into

into the earth, cannot fail of finding vent, and iffuing from many places in form of springs and fountains; and consequently there will be little or no water in mountains. In plains, on the contrary, as the water which filters into the earth cannot find vent. there will be masses of subterraneous waters in the cavities of the earth, and a great quantity of water which will sweat through the rocks of clay and close earth, or will be found dispersed and divided in the gravel and fand. It is this water which is found in low places; in common the bottom of a pit is nothing else but a bason in which the waters which isfue from the adjoining earth, collect together by falling at first drop by drop, and afterwards in little streams of water when the paths are open to the more remote waters; fo that it is right to fay, that although in low places we find water, we nevertheless can fink but fuch a number of wells, proportionate to the quantity of water dispersed, or rather to the extent of the higher land from whence these waters derive their fources.

In most plains, it is not necessary to dig to the level of the river to find water; it is commonly met with at a less depth, and there is no appearance that the waters of floods and rivers extend far by filtering through the earth. We must likewise not attribute to them the origin of all waters found below their level in the bowels of the earth, for in torrents and rivers which dry up, and in those whose course is turned, by digging in their bed, we never find more water than in the neighbouring lands; a narrow piece of land of five or fix feet thickness, is sufficient to contain the water and prevent it from escaping; and I have often observed, that the banks of brooks and pools are not fenfibly wet at fix inches distance. It is true, that the extent of filtration is more or less according as the foil is more or less penetrable: but, if we examine the hollows found in the earth earth and fand, we shall perceive that the water all paffes into the finall compass which it hollows of itfelf, and that the banks are scarcely level at a few inches distance in the fand; even in vegetable earth. where the filtration must be much greater than in the fand and other earth, fince it is affifted with the force of the capillary tube, we do not perceive it extend very far. In a garden we abundantly water and inundate one border without the earth, at a small diftance feeling any thing confiderable of it: I have remarked by examining large pieces of garden ground eight or ten feet thick, which had not been ftirred for feveral years, and whose surface was nearly level; that the pain had never penetrated deeper than three or four feet, fo that by turning up this earth in spring after a very wet winter, I found the earth as dry as when it had been heaped together. I made the same observation on earth collected for near two hundred years: below three or four feet the earth was dry as duft, fo that water neither communicated nor extended fo far as is thought by filtration alone. This mode only supplies the internal earth with the least part; but from the furface to great depths the water descends by its own weight; it penetrates by natural conduits or small paths which opens of itself; it follows the roots of trees, the cracks in rocks, the interflices in earth, and divides and extends on all fides into an infinity of small branches and fibres, always descending till it finds a vent, after having met with clay or fome other folid body on which it collects.

It would be very difficult to make a just valuation of the quantity of subterraneous waters which have no apparent vent. Many people have pretended that it greatly surpassed that of all the waters which are on the surface of the earth, and without speaking of those who have advanced, that the internal part of the globe was also lately filled with water. There are some who believe that there are an infinity of Vol. V.

floods, rivulets and lakes in the earth; but this opinion, although common, does not appear to me to be properly founded, and I think that the quantity ! of subterraneous waters, which have no iffue in the furface of the globe, is not very confiderable; for, if there were a great number of subterraneous rivers, why should we not see the mouths of some of these rivers on the furface of the earth? besides, rivers and all running waters produce very confiderable alterations on the furface of the earth; they sweep away the land with them, excavate rocks, and difplace all that opposes their passage. It would be the fame with fubterraneous rivers, they would produce fensible alterations in the internal part of the globe; but these alterations have never been observed. The strata subsists parallel and horizontal; the different matters every where preserve their primitive position, and it is only in very few places, that any confiderable veins of water has been observed. Thus water does act only in a small degree in the internal part of the earth, as it is divided into an infinity of little streams, as it is retained by so many obstacles; and in short, as it is dispersed almost every where, it immediately concurs to the formation of many terrestrial substances, which we must carefully diffinguish from ancient matters, and which, in fact, totally differs by their form and organization.

It is therefore the waters collected in the vast extent of the sea, which by the continual motion of the flux and re-flux, have produced mountains, vallies, and other inequalities in the earth; it is the currents of the sea which have hollowed vallies and raised hills by giving them correspondent directions. It is those waters of the sea, which by transporting earth have disposed them one on the other by horizontal layers; and it is the waters of heaven, which by degrees destroy the labour of the sea, which continually

lower mountains, fill up vallies, the mouth of floods, gulphs, and which bringing all to a level, will one day return this earth to fea, which will fue-ceffively be impaired, by leaving new continents bare, divide vallies and mountains, and all fimilar to those we at present inhabit.

PROOF of the THEORY of the EARTH.

where curcumits which is the state of the state which we have been already at the LE I. I ARTICLE is

Of the Formation of the Planets.

Our object being Natural History, we would willingly dispense with speaking of astronomy; but the nature of the earth depending on that of the heavens; and as we think to gain a greater intelligence of what has been said, it is necessary to give some general ideas on the formation, motion, and

figure of the earth and planets.

The earth is a globe of about three thousand miles diameter; it is fituate one thousand millions of miles from the fun, around which it makes its revolutions in 365 days. This revolution is the refult of two forces, the one an impulsion from right to left, or from left to right, and the other an attraction upwards or downwards, or the contrary towards a center. The direction of these two forces and their quantities are combined and proportioned in fuch a manner, that there refults an almost uniform motion in an eclipse very near to a circle. Like to the other planets the earth is opaque, it becomes dark, it receives and reflects the light of the one, and it turns round that planet according to the laws which agree with its distance and relative density. It also turns round its own axis once in twenty-four hours, and the axis around which this motion is made is inclined clined 66½ degrees on the plane of the orbit of its revolution. Its figure is spheriodical, the two axis of which differ about a 160th part, and the smallest axis

is that round which rotation is made.

These are the principal phenomena of the earth, these are the results of the great discoveries made by means of geometry, astronomy, and navigation. We shall not here enter into the detail they require to be demonstrated, and we shall not examine how we came to the knowledge of ascertaining the truth of these circumstances, it would be only repeating what has been already faid: we shall only make a few remarks, which may ferve to clear up what is still doubtful or contested, and at the same time, give our ideas on the formation of the planets, and the different states through which it is possible they have passed before they arrived at the state we at prefent see them. In the course of this work, we shall find extracts of so many systems and hypothesis's on the formation of the terrestial globe, the different states through which it has passed, and the charges which it has undergone, that it may not be thought amis, that we here join our conjectures to those of philofophers who have written on these matters, and especially when it is feen, that we in fact give these only as fimple conjectures, to which we do not only pretend to affign a greater degree of probability than to those made on the same subject; we do not the less refuse to publish what we have thought on this point, by which all hope to put the reader in a better state of pronouncing on the great difference there is between anhypothesis, &c. and theory founded on facts, between a fystem such as we shall present in this article on the formation, and the first state of the earth.

Galilée having found the rule of bodies, and Kepler having observed, that the area described by the principal planet around the sun, and those which the satellites describe around their principal planets, are proportionable to the time, and that the time of the revolutions of the planets and farellites are proportionable to the square roots of the cubes of their distances from the sun or principal planets. Newton found that the force which caused heavy bodies to fall on the furface of the earth, extended to the moon, and retained it in its orbit; that this force diminished in the same proportion as the square of the distance increases that consequently the moon is attracted by the earth, that the earth and planets are attracted by the fun, and that in general all bodies which describe a rounder center or focus proportional areas to the time, are attracted towards this point. This power which we are acquainted with by the name of gravity is therefore diffused throughout all matter; planets, comets, the fun, earth, and all nature is subject to its laws, and it serves as a basis to the universe: we have nothing better proved in physics than the actual and individual existence of this power in the planets, fun, earth, and in all matter that we touch or perceive. All observations have confirmed the actual effect of this power, and calculation has determined the quantity and relations of it; the exaness of geometricians and the vigilance of astromers with difficulty attain the precision of this celeftial mechanism, and the regularity of its effects.

This general cause being known, the phænomena are easily deduced from it, if the action of the powers which produce it are not too combined, but let us for a moment represent the system under this point of view, and we shall perceive what a chaos there has been to clear up. The principal planets are attracted by the sun, and the sun by the planets, the satellites are also attracted by their principal planets, and each planet is attracted by all the rest, and that attracts them likewise; all these actions and re-actions vary according to the masses and distances, they produce inequalities and irregularities: how is so great a number of connections

nections to be combined and estimated? Does it appear impossible in the midst of such a crowd of objects for to follow any particular one? Nevertheless, those difficulties have been surmounted, and calculation has confirmed the suppositions of reason; each observation is become a new demonstration, and the systematical order of the universe is laid open to the eyes of all those who know how to discover the tract.

One fingle thing stops us, in fact, independent of this theory; which is the force of impulsion, we evidently fee that the force of attraction always drawing the planets towards the fun, they would fall on a particular line on that planet, if they were not removed by another force, which can be only an impulsion in a right line, whose effects would be employed in the tangent of the orbit, if the force of attraction ceased one moment. The force of impulsion has certainly been communicated to the planets by the hand of the Almighty when he gave motion to the universe; but as we ought, as much as possible, to abstain in physics from having recourse to causes out of the road of nature; it appears that in the folar fystem we can give reason for this impulsive force in a very probable manner, and that we find a cause whose effect agrees with the mechanical tubes; and which in other respects does not remove the idea we ought to have on the subject of the changes and revolutions which may and must happen in the universe.

The vast extent of the folar system, which amounts to the same sphere of attraction of the sun does not confine itself to the orbs of the planets, but extends to a remote distance, always decreasing, in the same ratio as the square of the distance increases; it is demonstrated that the comets which are lost to our sight in the sky, obey this power, and that their motion, like that of the planets, depends on that of the sun; all these stars whose tracts are so different, deferibe around the sun, areas proportional to time.

The

The planets in elipsis's more or less approaching a circle comets in elongated ellipsis's. Comets and planets move therefore by virtue of two forces, the one of attraction, the other of impultion, which acting at one time, obliges them to describe these courses; but it must be remarked that the planets pass over the solar fystem in all manner of directions, and that the inclinations of their orbits are very different from each other, infomuch that, although subject like the planets, to the same force of attraction, the comets have nothing common in their motions of impulsion, they appear in this respect independent of each other: the planets, on the contrary, all turn around the fun in a like direction, and almost in the same plane, having there only 71 degrees of inclination between the planes, the most distant from their orbit; this conformity of position and direction in the motion of the planets, necessarily supposes somewhat common in this motion of impulsion, and must make us suspect that it has been communicated to them by one and the fame cause.

Can it not be imagined with some degree of probability, that a comet falling on the surface of the sun, will displace the planet and seperate from it some parts to which it communicates a motion of impulsion by a like direction and stroke, insomuch that the planets should formerly have belonged to the body of the sun, and been detached therefrom by an impulsive force common to all, and which they still preferve.

This appears to me at least as probable as the opinion of Leibnitz, who pretends that the planets and earth have been suns, and I think that his system, of which we shall find the precise account in the fifth article, would have acquired a great degree of generality, and somewhat more probability if he had raised himself to this idea. The case is here the same as with him, to think that the matter happened in the

darkness; for according to Leibnitz, light was divided from darkness when the planets were extinguished. But here the representation is physical and real, fince the opaque matter which composes the body of the planets, was really divided from the lumi-

nous matter which composes the fun.

This probability will augment prodigiously by the fecond analogy, which is, that the inclination of the orbits do not exceed 71 degrees; for by comparing the spaces, we shall find there is twenty-four to one, that two planets are formed in the most distant planes, and consequently 5, or, 7692624 to one, that it is not by chance that all fix are thus placed and thut up in the space of 71 degrees, or what amounts to the fame; there is a probability, that they have somewhat common in the motion which has given them this position. But what can they have in common in the impression of an impulfive motion, if it is not the force and direction of bodies which communicate it? it may therefore be concluded with a very great probability, that the planets received their motion of impulsion by one fingle froke. This probability, which is almost equivalent to a certainty, being acquired, I feek after what bodies in motion could make this stroke and produce this effect, and I see only the comets capable of communicating fo great a motion to fuch vast bodies.

Provided, we a little examine the course of comets, we shall be easily persuaded that it is almost necessary for them sometimes to fall into the sun. That of 1680 approached it so near, that its peritheliumwas not distant from it the sixth part of the solar diameter, and if it returns, as there is all appearance it will in 2255, it may then possibly fall into the sun. That depends on the rencounters it will meet with in its road, and of the retardment it suffered in passing

through

through the atmosphere of the sun. Vide Newton

3 edit. p. 525.

We may therefore prefume with this philosopher. that comets sometimes fall into the sun: but this fall may be made in different manners. If they fall directly in, or in a direction not very oblique, they will remain in the fun, and will ferve for food to the fire which the planet confumes, and the motion of impulsion which they will have loft, and communicated to the fun, will produce no other effect than that of displacing it more or less, according as the mass of the comet will be more or less considerable; but if the fall of the comet is made in a very oblique direction, which must oftener happen than in any other, then the comet will only graze the furface of the fun, or flightly furrow it; and in this case it may drive out some parts of matter to which it will communicate a common motion of impulsion, and these parts impelled out of the body of the sun and comet, might then become planets which will turn around this planet in the same direction and plane. We might perhaps calculate what mass, what velocity and what direction a comet should have to impel from the fun an equal quantity of matter to that which the fix planets and their fatellites contain; but this research would be here out of its proper place; it will be fufficient to observe, that all the planets with their fatellites, do not make the fixty-fifth part of the mass of the sun, (Vide Newton, p. 405,) because the denfity of the large planets. Saturn and Jupiter, is less than that of the sun, and although the earth be ten times and the moon near fix times denfer than the fun, they are nevertheless but as atoms in comparison of the mass of this planet.

I own, that however inconfiderable the 160th part is, at the first glance it appears to require a very powerful comet to seperate this part of the body of

the fun; but if we reflect on the prodigious velocity of comets in their perihelium, a velocity fo much the greater, as their track is more direct, and as they approach nearer the fun. If befides, we pay attention to the denfity, fixity and folidity of the matter of which they must be composed, to suffer, without being destroyed, the inconceivable heat they endure near the fun; and if at the fame time we reflect, they present to the fight of the observers a bright and folid body, which strongly reflects the light of the fun through the immense atmosphere of the comet which furrounds it, and must obscure it: we cannot doubt that the comets are not composed of a very folid and dense matter, and that they do not contain a great quantity of matter under a small volume; that confequently a comet cannot have fufficient mass and velocity to displace the sun, and give a projectile motion to a quantity of matter fo confiderable as is the 650th part of the mass of this planet. This perfectly agrees with what is known concerning the denfity of the planets: we think it is fo much the less as the planets are farther distant from the fun, and as they have less heat to support, so that Saturn is less dense than Jupiter, and Jupiter much less dense than the earth: and in fact, if the denfity of the planets was as Newton pretends, proportionable to the quantity of heat which they have to support, Mercury would be seven times denfer than the earth, and twenty-eight times denfer than the fun; the comet of 1680 would be 28000 times denfer than the earth, or 112000 times denfer than the fun, and by supposing it as large as the earth, it would contain nearly an equal quantity of matter to the ninth part of the fun, or by giving it only the rooth part of the fize of the earth, its mass would be still equal to the gooth part of the fun. From whence it is eafy to conclude, that fuch a mass as a small comet is, might seperate and drive

eafy

from the fun a 900th part, or a 650th part of its mass, particularly if we pay attention to the immense acquired velocity with which comets move when they

pass in the vicinity of the fun.

Another analogy which deferves some attention, is, the conformity between the denfity of the matter of the planets and the matter of the fun. We know on the furface of the earth there are some matters 14 or 15000 times denfer than others. The denfities of gold and air are nearly in this relation. But the internal part of the earth and the body of the planets are composed of more fimilar parts whose comparative denfity varies much lefs, and the conformity of the matter of the planets and of the denfity of the fun is fuch, that of 650 parts which compose the whole of the matter of the planets, there is more than 640 which are almost of the same denfity as the matter of the fun, and not ten parts out of these 650 of a greater density; for Saturn and Jupiter are nearly of the fame denfity as the fun, and the quantity of matter which these planets contain, is at least 64 times greater than the quantity of matter of the four inferior planets, Mars, the Earth, Venus and Mercury. We must therefore fay, that the matters of what the planets are generally composed of, is nearly the tame as that of the fun, and that confequently this matter may have been separated from it.

But it will be faid, if the comet by falling obliquely on the fun, has furrowed the furface of it, and drove off the matter which compose the planets, it appears that all the planets instead of describing circles of which the sun is the center, would, on the contrary, have razed the surface of the sun at each revolution, and would be returned to the same point from whence they departed, as every projectile would do which might be thrown off with sufficient force from the surface of the earth, to oblige it to turn perpetually: for it is

easy to demonstrate that this body would return at each revolution to the point from whence it would have been thrown off and hence we cannot attribute to the impulsion of a comet, the projection of the planets out of the sun, since their motion around this planet is difficult from what it would be in this hypothesis.

To this I reply, that the matter which composes the planets, is not come from that planet in ready formed globes, to which the planet has communicated its motion of impulsion; but, that this matter is come under the form of a torrent the motion of the anterior parts of which has been accelerated by that of the posterior parts: that befides the attraction of the anterior parts has also accelerated the motion of the posterior, and that this acceleration produced by one or the other of the causes, and perhaps by both, has been so great as to change the first direction of the motion of impulsion, and that a motion has resulted such as we at prefent observe in the planets, especially by suppoling that the stroke of the comet has displaced the fun: for, to give an example which will render this more reasonable, let us suppose, that from the top of a mountain a musket ball is discharged, and that the strength of the powder was great enough to fend it beyond the femi-diameter of the earth, it is certain, that this ball would turn round the point, and at each revolution would return to the from whence it had been discharged: but, if inflead a musket ball, we suppose a rocket had been discharged wherein the motion of the fire would be durable and greatly accelerate the motion of impulfion, this rocket, or rather the cartouch which contained it, would not return to the fame place like the musket ball, but would describe an orb, whose perigee would be much farther distant from earth, as the force of acceleration would be greater and have changed the first direction; all things being suppose ed equal in other respects. Thus, provided there had been any acceleration in the motion of impulsion communicated to the torrent of matter by the fall of the comet; it is very possible that the planets formed in this torrent, had acquired the motion which we know they have in the circles and ellipsis's of which the sun is the center and focus.

The manner in which the great erruptions of volcanos are made, may afford us an idea of this acceleration of motion in the comet we speak of. It has been observed, that when Vesuvius begins to roar and eject the matter it contains, the first matter has only a certain degree of velocity; but this velocity is soon accelerated by the impulsion of the second, which succeeds the first, since by the action of a third and so on, the heavy mass of bitumen, sulphur, einders, melted metal, &c. appear like massive clouds, and although they succeed each other nearly in like directions, they yet greatly change that of the first and drive it elsewhere and farther then it would have reached of itself.

In other respects, can we not answer to this objection, that the fun having been ftruck by the comet, and having received a part of its motion by impulsion, it will of itself have endured a motion which will have displaced it, and that although this motion of the fun, is at prefent too little fensible for aftronomers to be able to perceive it in small intervals of time; nevertheless, this motion may still exist and the fun move slowly towards different parts of the universe, by describing a curve around the center of gravity of the whole fystem? and, if this is fo, as I prefume it is, we fee perfectly that the planets, instead of returning near the fun at each revolution, will, on the contrary, have described orhits the points of the perihelia of which are fo much the

the farther diftant from this planet, as itself is from

the place it formerly occupied.

I am perfectly tentible that it may be faid, that if this acceleration of motion is made in the fame direction; it does not change the point of the perihelium which willalways be on the furface of the fun: but, must it be thought, that in a torrent, the parts of which succeed each other, there has been no change of direction: it is, on the contrary, very probable that there has been a very great alteration of direction, to give the planets the motion they have.

It may also be faid, that if the sun has been displaced by the stroke of a comet, it ought to move uniformly, and that hence this motion being common to every system, there was no need of any thing changing; but, might not the sun before the stroke, have had a motion around the center of gravity of the cometary system, to which primitive motion, the stroke of the comet will have added an augmentation or diminution? and that would suffice to render a

reason for the actual motion of the planets.

In short, if these suppositions are not admitted of, may it not be prefumed, without attacking probability, that in the stroke of the comet against the fun, there was an elaftic force which will have raifed the torrent above the furface of the fun, instead of directly impelling it? which alone may fuffice to throw off the point of the perihelium and give the planets the motion they have retained, and this supposition is not void of probability, for the matter of the fun may possibly be very elastic, fince the only part of this matter we are acquainted with, which is light, feems by its effects to be perfectly elastic. I own that I cannot fay whether it is by the one or the other of these reasons, that the direction of the first motion of the impulsion of the planets has changed, but they suffice to shew that this alteration is possible and even probable and that is also sufficient for my purpose. But,

But, without dwelling any longer on the objections which might be made, no more than on the proofs which analogies might furnish in favour of my hypothefis; let us purfue the object and deduce inductions from it : let us therefore fee what has happened when these planets and particularly the earth received this impulsive motion, and in what state they were found after having been separated from the fun. The comet having by a tingle stroke communicated a projectite motion to a quantity of matter equal to the 690th part of the fun, the less dense periheliums, will be seperated from the dense and will have formed by their mutual alteration globes of different denfities: Saturn composed of the most gross and light parts, will be the most remote from the fun: afterwards Jupiter who is denfer than Saturn will be less distant and so on. The larger and less planets are the most remote, because they have received an impulfive motion, stronger than the fmallest and densest: for, the force of impulsion communicating itself by furfaces, the fame stroke will have moved the groffer and lighter parts of the matter of the fun with more velocity than the fmallest and most massive; a seperation therefore will be made of the dense parts of different degrees, fo that the denfity of the fun being equal to 100, that of Saturn is equal to 67, that of Jupiter to 941 that of Mars to 200, that of earth to 400, Venus to 800, and Mercury to 2800. But the force of attraction not communicating like that of impulsion, by the furface and acting on the contrary on all parts of the mass it will have retained the densest portions of matter, and it is for this reason that the densest planets are the nighest the fun, and turn round that planet with greater rapidity than the less dense planets, which are also the most remote.

The two large planets, Jupiter and Saturn, which are, as is known, the principal planets of the folar

fystem,

fystem have retained this relation between their density and impulsive motions, in so just a proportion as must strike us; the density of Saturn is to that of Jupiter as 67 to $94\frac{1}{2}$ and their velocities are nearly as $88\frac{2}{3}$ to $120\frac{1}{72}$, or as 67 to $90\frac{1}{16}$ it is feldom that pure conjectures can draw such exact relations. It is true, that by following this relation between the velocity and density of planets, the den-

fity of the earth ought to be only as 2067 whereas it is as 400; from hence it may be conjectured, that our globe was formerly less dense than it is at present. With respect to the other planets, Mars, Venus and Mercury, as their denfity is known only by conjecture; we cannot know whether that would deftroy or confirm our opinion in relation of the velocity and denfity of the planets in general. The opinion of Newton is, that denfity is so much the greater, as the heat to which the planet is exposed is the stronger, and it is on this idea, that we have just said that Mars is once less dense than the earth, Venus once denser, Mercury feven times denfer, and the comet in 1688. 28000 times denfer than the earth; but this proportion between the denfity of the planets and the heat which they have to support, cannot subfift when we confider Saturn and Jupiter, which are the principal objects we should never lose fight of in the folar system: for according to this relation between the denfity and heat, it is found that the denfity of

Saturn would be about $4\frac{7}{18}$ and that of Jupiter as $14\frac{17}{22}$ instead of 67 and $94\frac{7}{2}$ too great a difference for the relation of density and heat the planets have to bear, to be admitted. Thus in spite of the considence which the conjectures of Newton merit, I think that the density of the planets, has more relation with their velocity than with the degree of

heat which they have to support. This is only a final cause and the other a physical relation, the preciseness of which is remarkable in the two large planets: it is nevertheless true that the density of the earth instead of being 206 \(\frac{1}{3} \) is found to be 400, and that consequently the terrestrial globe must be condensed in this ratio of 206 \(\frac{1}{3} \) to four hundred.

But the condensation or coction of the planets his it not fome relation with the quantity of the heat of the fun in each planet? and hence Saturn which is very distant from that planet will have fuffered little or no condensation. Jupiter will be condensed from 9016 to 941. Now the heat of the fun in Jupiter being to that of the fun upon the earth as 14% are to 400 the condensations ought to be in the same proportion of 2067 to 2151457 if it had been placed in the orbit of Jupiter, where it could have received from the fun a heat only equal to that which this planet receives, but the earth being nearer this planet and receiving a heat whose relation to that which Jupiter receives is from 400 to 142 the quantity of condensation must be multiplied which it would have had in the orb of Jupiter by the relation of 400 to 1423 which gives nearly 2341 for the quantity which the earth ought to be condensed. Its density was 2062, by adding thereto the quantity of condensation, we find 440 for its actual denfity, which nearly enough approaches the density 400, determined by the parallex of the moon. On the whole, I do not here pretend to give exact relations, but only approximations, to point out that that the denfity of the planets have much relation with their velocity in their orbits.

The comet having therefore by its oblique fall furrowed the furface of the fun, will have driven out of its body a part of matter equal to the 650th part of its whole mass; this matter which must be confidered in a state of sluidity, or rather of liquefaction, will at first have formed a torrent, the groffer and less dense parts of which will have been driven the farthest, and the densest parts having received only the like impulsion, will not be so very remote, the force of the fun's attraction having retained them. Every part detached by the comet and impelled one by the other will have been constrained to circulate around this planet, and at the fametime the mutual attraction of the parts of matter will have formed globes at different distances, the nearest of which to the fun will have necessarily retained more rapidity to turn perpetually afterwards round this planet.

But, it will be faid a fecond time, if the matter which composes the planets has been separated from the sun, the planets should be like the sun, burning and luminous, and not cold and opaque as they are: nothing resembles this globe of fire less than a globe of earth and water, and to judge of it comparison, the matter of the earth and planets

is perfectly different from that of the fun.

To this it may be answered, that in the seperation which was made of the more or less dense particles, the matter has changed form and the light or fire is extinguished by this separation caused by the motion of impulsion. Besides, may it not be suspected that if the sun, or a burning or luminous star moves of itself with so much velocity as the planets move, the fire would be extinguished perhaps, that is the reason why that all luminous stars are fixed, and that those stars which are called new, and which have probably changed place, are extinguished from fight?

this is confirmed by what has been observed on comets, they must burn to the center when they pass to their perihelium. Nevertheless they do not become luminous of themselves, we see only that they exhale burning vapours of which they leave a confi-

derable part by the way.

I own, that if fire can exist in a medium where there is very little or no refistance, it would also suffer a very great motion without extinguishing: I also own, that what I have just faid must extend only to the stars which totally disappear, and that those which have periodical returns and which flew themselves and difappear alternatively without changing place, are very difficult from those I speak of. The phenomena of these singular planets has been explained in a very fatisfactory manner by M. de Maupertuis, in his discourse on the figure of the planets, and I am convinced that by quoting facts which are known to us, it is not possible to devine them better than he has done. But the stars which appear and afterwards disappear entirely, are probably extinguished, either by the velocity of their motion, or by fome other cause, and we have no example in nature, that one 'luminous planet turns round another. Among twenty-eight or thirty comets, and thirteen planets which compose our fystem, and which move round the fun with more or less rapidity, there is not one luminous of itself.

It might be still answered, that fire cannot subfist fo long in the small as in large masses, and that at the departure from the fun the planets must have burnt for some time, but that they were extinguished for want of combustible matters, as probably the fun will be extinguished for the same reason; but in future, and in as distant ages from the time of the extinction of the planets as its fize is to that of the planets. Be it as it may, the seperation of the more or less dense parts, which is necessarily made at the time that the comet has driven the matter of the planets

out of the sun, appears to me sufficient to render

reason for this extinction of their fires.

The earth and planets therefore at the time of their quitting the fun, were burning and in a state of total liquefaction; this state remained only as long as the violence of the heat which had produced it : by degrees the planets cooled, and it was in this state of fluidity, caused by the fire, that they took their form, and that their motion of rotation raised the parts of the equator by lowering the poles. This figure, which agrees so perfectly with the laws of hydroftatics, necessarily supposes that the earth and planets have been in a state of fluidity, and I here follow Leibnitz's opinion, that though fluidity was a liquefaction caused by the violence of the heat, the internal part of the earth must be a vitrifiable matter, of which fand, gravel, &c. are the fragments and fcoria.

It may therefore with some probability be thought, that the planets appertained to the sun, that they were seperated by a single stroke which gave to them a motion of impulsion in the same direction and plane, and that their position at different distances from the sun, proceeds only from their different densities. It now remains to explain by the like theory, the motion of the rotation of the planets, and the formation of the satellites; but this, far from adding difficulties or impossibilities to our hypothesis, seems on

the contrary, to confirm it.

For the motion of rotation depends folely on the obliquity of the stroke, and it is necessary that an impulsion when it is oblique to the surface of a body, gives it a rotative motion: this motion will be equal and always the same, if the body which receives it is homogenous, and it will be unequal if the body is composed of heterogenous parts, or of different densities; and hence we must conclude, that in every planet the matter is homogenous, since their

the motions are equal. Another proof of the seperation of the dense and less dense parts when they are formed.

But the obliquity of the stroke might be such, as to seperate from the body of the principal planet a fmall part of matter, which will preserve the same direction of motion as the principal planet itself; these parts will be united according to their densities, at different distances from the planet by the force of their mutual attraction, and at the same time, neceffarily follow the planet in its course around the fun, by turning themselves around the planet, nearly in the plane of its orbit. We see plainly, that those small parts which the great obliquity of the stroke will have seperated, are the fatellites: thus the formation, position, and direction of the motions of the fatellites perfectly agree with theory; for they have all the fame motion in concentrical circles round their principal planet; their motion is in the fame plane, and this plane is that of the orbit of the pla-All these effects which are common to them. and which depend on their motion of impulsion, can proceed only from one common cause, i. e. from a common impulsion of motion, which has been communicated to them by one and the same stroke, given under a certain obliquity.

What we have just said on the cause of the motion of rotation and formation of the satellites, will acquire more probability if we consider all the circumstances of the phenomena. The planets which turn the swiftest on their axis, are those which have satellites. The carth turns quicker than Mars in the relation of about 24 to 15; the earth has a satellite, but Mars has none. Jupiter particularly, whose rapidity round its axis is five or six hundred times greater than that of the earth; has four satellites, and there is a great appearance that Saturn, which

has five, and a ring, turns still much quicker than

Jupiter.

It may even be conjectured with fome foundation, that the ring of Saturn is parallel to the equator of this planet, so that the plan of the equator of the ring and that of the equator of Saturn, are nearly the fame; for by supposing, according to the preceding theory, that the obliquity of the stroke by which Saturn has been fet in motion was very great; the velocity around the axis which will have refulted from this oblique stroke, will at first have been such as the centrifugal force exceeds that of the gravity. and there will be detached from the equator and its neighbouring parts, a confiderable quantity of matter which will necessarily have taken the figure of a ring, whose plane must be nearly the same as that of the equator of the planet; and this part of matter which forms the ring, having been detached from the planet in the vicinity of the equator, Saturn has been fo much lowered under the equator, which causes, that notwithstanding its rapidity, the diameters of this planet cannot be fo unequal as those of Jupiter, which differs more than an eleventh part.

However great the probability of what I have advanced hitherto on the formation of the planets and their fatellites, may appear in my fight, as each has his particular measurement, especially to estimate probabilities of this nature, and as this measurement depends on the strength of the understanding to combine more or less distant relations, I do not pretend to constrain the incredulous. I have only thought it my duty to sow these ideas, because they appear to me reasonable and proper to clear up a matter, on which nothing has ever been written, however important the subject is, since the motion of impulsion of the planets enters at least as one half in the composition of the universe, which attraction alone cannot unfold. I shall only

add the following questions to those who would deny the possibility of my system.

1st. Is it not natural to imagine, that a body in motion has received that motion by the stroke of

another body?

and. Is it not very probable that many bodies which have the fame direction in this motion, have received this direction by one fingle, or by many strokes directed in the same direction?

3rd. Is is not quite probable, that many bodies having the fame direction in their motion, and their position in a like plane have not received this direction in the same manner, and this position in the same plane by many strokes, but by one and the same stroke.

4th. Is it not very probable that at the same time that a body receives a motion of impulsion, it receives it obliquely, and that consequently it is obliged to turn on itself so much the quicker, as the obliquity of the stroke will have been greater? if these questions should not appear unreasonable, the system of which we have presented the outlines will cease to

appear an abfurdity.

Let us now pass on to something which more nearly concerns us, and examine the figure of the earth, on which so many researches and such great observations has been made. The earth being as it appears by the equality of its diurnal motion and the constancy of the inclination of its axis, composed of homogenous parts, and all its parts attractory to each other in ratio of their masses; it would necessarily have taken the figure of a globe perfectly spherical, if the motion of impulsation had been given it in a perpendicular direction to the surface; but this stroke having been obliquely given, the earth has turned on its axis at the same time as it took its form: and from the combination of this rotative motion and that of the attraction of the parts, there has resulted a spheroid

figure more elevated under the great circle of rotation, and lower at the two extremities of the axis, and this because the action of the centrifugal force proceeding from the rotation, diminishes the action of gravity. Thus the earth being homogenous and having taken its confistency at the same time it received its rotative motion, it took a spheroid figure, the two axis of which differ a 230th part. This may be exactly demonstrated, and does not depend on hypothesis's made on the direction of gravity; for it is not allowable to form hypothesis's contrary to established truth, or what may be established; now the laws of gravity are known to us, we cannot doubt, but that bodies weigh one on the other in a direct ratio of their masses, and in an inverted ratio of the square of their distances : so likewife we cannot doubt, that the general action of any mass is not composed of all the particular actions of that mais. Thus there is no hypothesis's to form on the direction of gravity; each part of matter mutually attracts in a direct ratio of its mass and an inverted ratio of its diffance, and from all these attractions there refults a fphere when there is no rotation, and a spheroid when there is one. This spheroid is longer or shorter at the two extremities of the axis of rotation, in proportion of the velocity of this motion, and the earth has taken by virtue of its rotative velocity and of the mutual attraction of all its parts, the figure of a spheroid, the two axis of which are as 229 to 230 between them.

Thus by its original constitution, by its homogenousness, and independent of every hypothesis or the direction of gravity, the earth has taken this figure at its formation, and it is by virtue of mechanical laws, raised about 6½ miles higher at each extremity of the

diameter of the equator than under the poles.

I shall dwell on this article, because there are still geometricians who think that the sigure of the earth in theory depends on the system of philosophy that is embraced. braced, and the direction which we suppose to gravity. The first thing we have to demonstrate, is the mutual attraction of every part of matter, and the second the homogenousness of the terrestrial globe: if we clearly point out, that these two circumstances cannot be revoked, there will no longer be any hypothesis to be made on the direction of gravity: the earth will necessarily have had the sigure given it by Newton, and every other sigure given to it by virtue of vortixes or other hypo-

thefis, will not be able to fubfist.

It cannot be doubted, unless the whole is, that it is the force of gravity which retains the planets in their orbits; the fatellites of Saturn gravitate towards Saturn, those of Jupiter towards Jupiter, those of the moon towards the moon; and Saturn, Jupiter, Mars, the earth, Venus and Mercury, gravitate towards the fun: fo likewife Saturn and Jupiter gravitate towards their fatellites, the earth gravitates towards the moon, and the fun towards the planets. Gravity is therefore general and mutual in all the planets, for the action of one force cannot be exercifed without a reaction; all the planets therefore act mutually one on the other. This mutual attraction serves as a foundation to the laws of their motion, which is also demonstrated by phenomena. When Saturn and Jupiter are in conjunction, they act one on the other, and this attraction produces an irregularity in their motion round the fun. It is the fame with the earth and the moon, they act mutually one on the other; but the irregularities of the motion of the moon, proceeds from the attraction of the fun, fo that the earth, the fun and the moon, mutually act one on the other. Now this mutual attraction of the planets is proportional to their quantity of matter when the distances are equal, and the same force of gravity which causes heavy matter to fall on the furface of the earth, and which extends to the Vol. V. Bbb moon, moon, is also proportional to the quantity of matter; therefore the total gravity of a planet is composed of the gravity of each of the parts which compose it; therefore all the parts of matter either in the earth or in the planets, gravitate towards each other; therefore all parts of matter mutually attract each other; and this being once proved, the earth by its rotation has necessarily taken the figure of a spheroid, the axis of which are between each other, as 229 to 230, and the direction of the weight is necessarily perpendicular to the furface of the spheroid; consequently there is no hypothesis to be made in the direction of the weight, at least as the heat and general attraction of the parts of matter is not densed; but we have just perceived that the mutual attraction is demonstrated by observations, and the experiment of pendulums prove, that it is general in all parts of matter; therefore we cannot make new hypothefis's on the direction of weight without going against experience and reason.

Let us now proceed to the hemogenousness of the terrestrial globe. I own, that if it is supposed that the globe is denfer in some parts than in others, the direction of gravity must be different from that we have just affigned, that it will be different according to the different suppositions that will be made, and that the figure of the earth will become different also by virtue of the like suppositions. But what reason have we to think it is so? why, for example, is it that the parts near the center are denfer than those which are more remote? all the particles which compose the globe, are not they collected together by their mutual attraction? hence each particle is a center, and there is no reason to believe, that the parts which are about the center of the globe, are denfer than those which are about any other point. But besides, if one considerable part of the globe was denfer than another, the axis of rotation would be found near the dense parts, and an inequality would enfue in the diurnal revolution, fo that at the furface of the earth, we thould remark an inequality in the apparent motion of the fixed stars, they would appear to move much quicker, or much flower towards the zenith, than the Horizon: according as we should be placed on the denfer or lighter parts of the globe, this axis of the earth no longer passing thro' the center of the globe, would also very fensibly change position: but all this does not happen, on the contrary, it is known that the diurnal motion of the earth is equal and uniform: it is known that at all the parts of the earth's furface, the stars appear to move with the fame velocity at all heights, and if then as a nutation in the axis, it is infenfible enough to have escaped observers: it must therefore be concluded, that the globe is homogenous, or nearly fo in all its parts.

If the earth was a hollow and void globe, the crust of which, for example, would be only two or three miles thick: there would refult, 1. That the mountains would be in this case such confiderable parts of the whole thickness of the crust, that there would be great irregularity in the motions of the earth by the attraction of the moon and fun: for when the highest parts of the globe as the Cordilliers, should have the moon at noon, the attraction would be much stronger on the whole globe than when the lowest parts should have this planet at noon. 2. The attraction of mountains would be much more confiderable, then it is in comparison of the whole attraction of the globe, and experiments made at the mountain of Chimboraco in Peru, would in this cafe give more degrees than they have given feconds for the deviation of the plumb line. 3. The weight of bodies would be greater on the top of an high mountain, as the Pike of Teneriff, than at the level of the fea; fo that we should feel ourselves confiderably heavier and should walk more difficulty in high places than in low. These considerations convince, and some more which may be added must, that the inner part of the globe is not void, but

filled with a denfe matter.

On the other hand, if below the depth of two or three miles, the earth was filled with a matter much more dense than any other known matter; it would necesfarily occur, that every time we descended to moderate depths, we should weigh much more, the pendulums would be more accelerated, than in fact they are when carried from an high plane to a low: thus, we may prefume that the internal part of the earth is filled with a matter nearly fimilar to that which composes its surface. What may compleat our determination in favour of this opinion is, that in the first formation of the globe, when it took the form of a flat spheroid under the poles, the matter which composed it was in fusion and consequently homogenous and nearly equally dense in all its parts, as well on the furface as on the infide. From that time the matter of the furface, although the fame, has been flirred and worked by external causes, which has produced matters of different denfities; but, it must be remarked, that the matters, which like gold and metals, are the denfest, are also those the most feldom met with, and that in confequence of the action of external causes, the greatest part of the matter which composes the furface of the globe, has not undergone any very great changes with relation to its denfity, and the most common matters, as fand and clay, do not differ much in denfity, infomuch, that there is all the room imaginable to conjecture with great probability, that the internal part of the earth is filled with a vitrified matter, the denfity of which is nearly the fame as that of fand, and that confequently the terreftrial globe in general may be regarded as homogenous. There

There remains a refource to those who would absolutely make suppositions, which is to say, that the globe is composed of concentrical strata of different densities; for, in this case, the diurnal motion will be equal and the inclination of the axis constant as in the case of homogenousness. I acknowlege it, but I ask at the same time, if there is any reason to believe that strata of different densities do exist, if it is not to desire the works of nature to adjust themselves to our ideas, and if in physics we must admit of a supposition which is not sounded on any observation or analogy and which does not agree with any of the inductions which we may draw from elsewhere.

It appears therefore, that the earth has taken by virtue of the mutual attraction of its parts and of its rotation the figure of a fpheroid whose two axis differ one 230th part; this appears to be the primitive figure, that it necessarily took in its state of sluidity or liquification: it appears, that by virtue of the laws of gravity and the centrifugal force, it can have no other figure: that, in the moment even of its formation, there was this difference between the two diameters, of 6 an half miles of elevation more under the equator than the poles, and that consequently every hypothesis in which we can find greater or less difference are sictions to which we must pay no attention.

But it will be faid, if this theory is true, if the relation of 229 to 230 is the true relation of the axis, why do the mathematicians fent to Lapland and Peru, agree to the relation of 174 to 175? Fromwhence does this difference between theory and practice come? And without injuring the reason just made to demonstrate the theory; is it not more reasonable to give the preference to practice and measares, especially when we cannot doubt that they have been taken by the most able mathematician of Europe, (M. de Maupertius figure of the Earth) and with all necessary precautions to establish the result?

To this I answer, that I have taken care to pay attention to the observations made under the equator, and Polar circle: that I have no doubt of their being exact, and that the earth may possibly be elevated a 175th part more under the equator than the poles. But, at the fame time I maintain the theory, and I fee clearly the two refults may be reconciled. This difference is about four miles in the two axis's, fo that the parts under the equator are raifed two miles more than they ought to be according to theory; this height answers exact enough to the greatest inequalities of the the furface of the globe, they proceed from the motion of the fea, and the action of the fluids, on the furface earth. I will explain myfelf, it appears that when the earth was formed, it must necessarily have taken, by virtue of the mutual attraction of its parts, and the action of the centrifugal force, a spheroidical figure, the axis of which differs a 230th part: the old and original earth has necessarily had this figure which it took when it was fluid, or rather liquified by the fire: but when after its formation and refrigeration, the vapours which were extended and rarefied; as we fee the atmofphere and tail of a comet is become condensed and fell on the furface of the earth, and formed air and water: and when thefe waters which were on the furface, became agitated by the flux and reflux, the matters were by degrees carried from the Poles towards the equator: fo that it is possible, that the parts of the Poles are lowered about a mile, and those of the equator raifed in the same quantity; this was not fuddenly done, but by degrees in fuccession of time, the earth being extremely exposed to the winds, air, and fun; all these irregular causes concurred with the flux and reflux to furrow its furface, hollow it into depths, and raife it into mountains which has produced inequalities and irregularities in this state of disturbed earth, of which, nevertheles

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greatest thickness can only be about one mile under the equator: this inequality of two miles, is perhaps the greatest which can be to the surface of the earth, for the highest mountains are scarcely above one mile, and the greatest depths of the sea are perhaps not one mile. The theory is therefore true, and practice may be fo likewife; the earth at first was only raised about 61 miles more under the equator than the poles, and afterwards by the changes which have happened to its furface, it has been raifed still more. Natural History wonderfully confirms this opinion, and we have proved in the proceeding difcourfe that the flux and reflux, and other motions of the water, which have produced mountains and all the inequalities on the furface of the globe: that this fame furface has undergone very confiderable changes, and that at the greatest depths, as well as on the greatest heights, bones, shells and other wrecks of animals, which inhabit the fea and earth, are met

It may be conjectured by what has been faid, that to find the ancient earth, and the matters which have never been stirred, we must dig into the parts of the pole, where the state of the earth must be thinner than in the Southern climates.

On the whole if we examine the measures by which determined the figure of the earth is, we shall perceive, that hypothesis enters into such determination, for it supposes the earth to have the figure of a regular curve, whereas it may be thought that the surface of the globe, having been changed by a great quantity of causes combined ad infinitum; it perhaps has no regular figure, and hence the earth might possibly be in fact only flattened a 230th part as Newton says, and as theory requires. Besides it it well known, that altho' it has exactly the length of the degree at the polar circle and equator, we have not the length of the de-

gree so exactly in France, and the measure of M. Picard has not been verified : add to this, that the augmentation and diminution of the pendulum cannot agree with the refult of measures, and that, on the contrary, they agree very little with Newton's theory. Hence there is more than requifite for us to fuppose, that the earth is really flattened only a 230th part, and that if there is any difference, it can proceed only from the inequalities which the water and other external causes have produced on its surface; and these inequalities being according to all appearance more irregular than regular, we must form an hypothefis thereon, nor suppose, as has been done, that the meridians are ellipsis's or some other irregular curves. From whence we perceive, that when we would fucceffively measure many degrees of the earth in all directions, we shall still not be certain, by that of the quantity of flatness, that it can have more or less than the 230th part.

Must it not be also conjectured, that if the inclination of the axis of the earth has changed, it can only be by virtue of the changes happened to the surface, since all the rest of the globe is homogenous; that consequently this variation is too little sensible to be perceived by astronomers, and that at least as the earth has not encountered with any comet, or deranged by any other external cause, its axis will remain perpetually inclined as it is at present, and

as it has always been.

And in order not to omit any conjectures which appeared to me reasonable, may it not be said, that as the mountains and inequalities which are on the surface of the earth have been formed by the flux and re-flux; the mountains and inequalities which we remark on the surface of the moon has been produced by a similar cause; that they are much higher than those of the earth, because the flux and re-flux is much stronger there; since it is the moon here,

and these it is, caused by the earth, which cause being much more considerable than that of the the moon must produce much greater effects, if the moon had, like the earth, a rapid rotation by which it would successively present to us all the parts of its surface: but as the moon presents always the same surface to the earth, the slux and re-slux cannot operate on that planet, but by virtue of its motion of libration by which it alternatively discovers to us a segment of its surface, which must produce a kind of slux and re-slux, quite different from that of our sea, and the effects of which must be much less considerable than they would be, if this motion had from its course a revolution of this planet round its axis as quick as the rotation of the terrestrial globe.

I should furnish a book as large as that of Burnet or Whiston's, if I chose to enlarge the ideas which compose the system just laid down, and by giving them a geometrical air, as this last author has done, I should have given them at the same time weight: but I think that Hypothesis's, however probable they are, must not be treated with that dress

which borders a little on quackery.

PROOF of the THEORY of the EARTH.

ARTICLE II.

* From the System of Whiston.

THIS Author commences his treatife by a differtation on the creation of the world: he fays, that the text of Genefis has been but badly understood; that the translators have confined themselves too much to the letter and sense which offers itself at first fight, without considering what nature, reason, Vol. V. Ccc philo-

^{*} A new Theory of the Earth, by William Whiston, 1708.

philosophy, and even decency exacts from the writer, to treat this matter in a proper manner. He fays, that the common motion of the world being made in fix days is absolutely false, and that the description given by Moses, is not an exact and philosophical narration of the whole universe and origin of things, but an historical representation of the formation of the terrestrial globe only. The earth, according to him, existed in the chaos; and in the time mentioned by Moses, received the form, situation, and confistency necessary to be inhabited by the human race. We shall not enter into a detail of his proofs in this respect, nor undertake their resutation. The exposition we have just made, is sufficient to demonstrate the difference of his opinion with public facts, and consequently the insufficiency of his proofs. On the whole, he treats this matter as a theological controvertist, rather than as an enlightened philosopher.

Leaving principles, he flies to ingenious suppofitions, and which, although extraordinary, yet have a degree of probability to those, who like him, incline to the enthusiam of system. He says, that the chaos, the origin of our earth, has been the atmofphere of a comet: that the annual motion of the earth began at the time it took a new form, but that its diurnal motion began only when the first man fell. That the eliptic circle then cut the tropic of cancer, at the point of the terrestrial paradife, on the north-west side of the frontiers of Assyria: that before the deluge the year began at the autumnal equinox: that the original orbits of the planets, and especially that of the earth, were perfect circles before the deluge. That the deluge began the 18th of November 2765, of the julian period, i. e. 2349 years before Christ. That the julian year and the lunar year were then the same, and that they exactly contained 360 days. That a comet descending in

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the plane of the eliptic towards its perihelium, paffed near the globe of the earth the fame day as the deluge began: that there is a greater heat in the internal part of the terrestrial globe, which constantly diffuses itself from the center to the circumference; that the internal and total conflitution of the earth is like that of an egg, the ancient emblem of the globe: that mountains are the lightest part of the earth, &c. He afterwards attributes all the alterations and changes which have happened to the furface and internal part of the globe, to the univerfal deluge. He blindly adopts the hypothesis's of Woodward, and indistinctly makes use of all the obfervations of this author, on the present state of the globe; but he fubjoins much when he speaks of the future state of the earth: according to him it will be confumed by fire, and its destruction will be proceeded by terrible earthquakes, thunder, and frightful meteors; the fun and moon will have an hideous afpect, the heavens will appear to fall, and the flames will be general over all the earth: but when the fire shall have devoured all that it contains impure; when it shall be vitrified and transparent as christal, the faints and the bleffed will return and take poffeffion of it, and inhabit it till the day of judgment.

All these hypothesis's at the first glance appear to be so many rash, not to say extravagant affertions: nevertheless, the author has managed them with such address, and treated them with such strength, that they cease to appear absolutely chimerical. He gives to his subject as much spirit and knowledge as he could, and it will be ever surprizing, that from a mixture of ideas so very absurd, a system could be found to mislead mankind. It has not affected vulgar minds so much as it has dazzled the eyes of the learned, because they are more easily disconcerted than the vulgar by the stamp of erudition, and by the power and povelty of ideas. Our author was a celebrated astro-

nomer; he could never persuade himself that this fmall grain of fand, this earth which we inhabit, had attracted the attention of the Creator fo greatly, as to occupy it longer than the heaven and the universe, the vast extent of which contains millions of millions of fouls and earths. He pretends therefore, that Moses has not given us the history of the first creation, but only the detail of the new form that the earth took when the hand of the Almighty drew it from the mass to make it a planet; or what amounts to the same, when from a world of disorder and unshapen, Chaos formed it into a habitation, and an agreeable abode. The comets are. in fact, subjected to terrible vicisfitudes by reason of the excentricity of their orbits. Sometimes, like that in 1680, it is a thousand times hotter there than in the midst of a red hot stove: sometimes it is colder than ice, and they can be inhabited only by strange creatures.

The planets, on the contrary, are places of rest where the distance from the sun, not varying much, the temperature remains nearly the same, and permits different kinds of plants and animals to grow,

last, and multiply.

In the beginning God created the world; but according to our author, the earth, confounded with other excentric stars, was then an inhabitable comet, suffering alternatively the excess of heat and cold, in which matters, liquifying, vitrifying and freezing by turns, formed a chaos, an abyss, surrounded with thick darkness: & tenebræ erant superfaciem abyss. This chaos was the atmosphere of the comets, which must be represented as a body composed of heterogeneous matters, the center occupied by a spherical, solid and hot substance, of about two thousand miles diameter, around which every great surface of a thick sluid extended, mixed

mixed with an unshapen and confused matter, like

the ancient chaos, rudis indigestaque moles.

This vast atmosphere contained but very few dry, folid, or terrestrial parts, still less aqueous or aerial particles; but a great quantity of fluid, dense and heavy matters, mixed, agitated and confounded together. Such was the earth before the fix days, but on the first day of the creation, when the excentrical orbit of the comet had been changed. every thing took its place, and bodies arranged themselves according to the law of gravity, the heavy fluid descended to the lowest places, and left the upper region to the terrestrial, aqueous and acrial parts : those likewise descended according to their order of gravity : first, the earth ; then the water, and last of all the air. This sphere of an immense chaos, reduced itself to a globe of a moderate fize, in the center of which is the folid body, which to this day retains the heat which the fun formerly communicated to it, when it belonged to a comet. This heat may possibly endure fix thoufand years. Since the comet of 168 required fifty thousand years to cool in, and that in passing by its perhielium, it endured a heat two thousand times ftronger than that of red hot iron. Around this folid and burning matter which occupies the center of the earth, the denfe and heavy fluid which defcended the first is to be found, and this is the fluid which forms the great abyss on which the earth is borne, like cork on quickfilver: but as the terrestrial parts were mixed with much water, in descending they have dragged with them a part of this water, which has not been able to reascend after the earth This water formed a concentriwas confolidated. cal bed with the heavy fluid which furrounds this hot fubstance, insomuch that the great abysis is compassed with two concentrical orbs, the most internal of which is a heavy fluid, and the other water. It is properly this this bed of water which ferves for a foundation to the earth, and it is this admirable arrangement of the atmosphere of the comet, in which the theory of the earth and the explanation of the phenomena

depend.

For we know that when the atmosphere of a comet was once disembarrassed from all these solid and terrestrial matters, there remains only the lighter matter of the air, through which the rays of the fun freely passed, and which, all at once, produced light; fiat lux. We fee the columns which composed the orb of the earth, being formed with fuch great precipitation, are found of different denfities, and that confequently, the heaviest has funk deeper into this Subterraneous fluid; and it is this which has produced valleys and mountains on the furface of the earth. These inequalities were, before the deluge, dispersed and fituated otherwise than they are at prefent. Inflead of the vast valley which the ocean contains, there were many fmall divided cavities on the furface of the globe, each of which contained a part of this water, and formed fo many fmall particular feas: the mountains were also more divided, and did not form chains as they at present do: nevertheless, the earth was a thousand times more peopled, and confequently a thousand times more fertile than it now is; the life of man and animals were a thousand times longer, and all that, because the internal heat of the earth which proceeded from the center, was then in its full power; and that this greatest degree of heat, unfolded and shot out a greater number of plants and animals, and gave them a degree of vigour, necessary for them to subfift a long time, and multiply in greater abundance. But this heat, by increasing the strength of bodies, unfortunately tended to the heads of men and animals; it augmented their passions; it deprived animals of wisdom and men of innocence; all, except fifh,

fish, which inhabit in a cold element, feel the effects of this heat; at length all became criminal and merited death. It therefore came, and this universal death happened on a Wednesday, the 28th of November, by a terrible deluge of forty days and forty nights, and this deluge was caused by the tail of another comet, which encountered the

earth in returning from its perihelium.

The tail of a comet is the highest part of its atmosphere; it is a transparent mist, a fable vapour, which the heat of the fun exhales from the body and atmofphere of the comet: this vapour composed of extremely rarefied aqueous and aerial particles, follows the comet when it descends to its perihelium, and precedes when it re-ascends, so that it is always situate opposite to the fun, as if it fought to be in the shade, and avoid the too great heat of that luminary. The column which this vapour forms is always of an immense length, and the more a comet approaches the fun, the longer and mere extended is its tail, fo that it often occupies a very great space, and as many comets descend below the annual orb of the earth: it is not furprifing that the earth is fometimes found furrounded with the vapour of this tail: this is precifely what happened at the time of the deluge. Two hours flay in this tail of a comet is sufficient to throw down as much water as there is in the sea. In short, the tail was the cataract of heaven, & cataracti cali aperti funt. In fact, the terrestrial globe, having once met with the tail of the comet, it must, in going its course, appropriate to itself a part of the matter which it contains; all which is to be found in the sphere of the attraction, the globe must fall on the earth, which must fall in the form of rain, fince this tail is partly composed of aqueous vapours. This therefore is a rain which may be thrown down abundantly; this is an universal deluge, the waters of which

which will eafily furmount the highest mountains. Nevertheless our author who in this part of his work, will not quit the holy writ; does not say that this rain was the sole cause of the universal deluge, he takes the water from every place it is in: the great abyss, as we see contains a good quantity of it, the earth at the approach of the comet will without doubt, have proved the force of its attraction; the liquids contained in the great abyss will have been agitated by so violent a flux and reflux, that the superficial crust could not resist, but split in several places, and the internal waters were dispersed over the surface,

& rupti funt fontes Abyffi.

But what became of these waters, which the tail of the comet, and the great abysis furnished so liberally? Our author is not the least embarrassed thereon: as foon as the earth continuing its course, was removed from the comet, the effect of its attraction, the motion of flux and reflux ceased in the great abyss: and hence the upper waters were precipitated there with violence by the fame roads as they The great abyss absorded all the were driven. fuperfluous waters and was of a capacity large enough not only to receive the waters which it already contained; but also, all those which the tail of the comet had left, because during the time of its agitation, and the rupture of its cruft, it had enlarged the space by driving on all fides the earth which furrounded it. It was in this time also, the figure of the earth, which till then was fpherical, became elliptic, as well by the effect of the centrifugal force, caused by its diurnal motion, as by the action of the comet: and that because the earth in running over the tail of the comet, found itself placed so that it presented the parts of the equator to that planet, and that the force of the attraction of the comet concurring with the centrifugal force of the earth, caused the parts of the equator to be elevated; with the

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the more facility, as the crust was broken and divided into an infinity of parts, and as the action of the flux and re-flux of the abyss, drove the parts of

the equator more violently than elfewhere.

Here then is a history of the creation; the causes of the univerfal deluge; the length of the life of the first Man; and the figure of the earth; all this feems to have cost our author little or no labour; but Noah's ark, appears to have greatly disquieted him: how in fact to imagine that in the midft of so terrible a disorder; in the midst of the confusion of the tail of a comet with the great abysis; in the midst of the ruins of the terrestrial globe, and in the terrible moments wherein not only the elements of the earth were confused: but where new elements still concurred to augment the chaos; how to imagine that the Ark floated quietly with its numerous cargo on the top of the waves! Here our author makes great efforts to arrive at, and give a physical reason for the prefervation of the ark; but it has appeared to me infufficient, poorly imagined, and but little orthodoxical, I will not here relate it; it will be fufficient for me to point out how hard it is for a man who has explained fuch great things, without having recourfe to a supernatural power or a miracle. to be stopt by one particular circumstance; so our author chose rather to drown himself with the ark. than to attribute to the immediate bounty of the almighty, the prefervation of this precious veffel.

I shall only make one remark on this system, of which I have just made a faithful exposition; which is, that every time that we are rash enough to attempt to explain theological truths, by physical reasons, or interpret purely by human views, the divine text of holy writ, or that we endeavour to reason on the will of the most high, and on the execution of his degrees; we consequently shall fall into the darkness and chaos where the author of this system is

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fallen into; and which nevertheless, has been received with great applause. He neither doubts of the truth of the deluge, nor of the authenticity of the sacred writ: but, as he was less employed with it, than with physic and astronomy: he has taken the passages of the scripture for physical circumstances, and the results of astronomical observations; and has so strangely blended the divine knowledge with human, that the most extraordinary matter in the world has resulted therefrom, which is the system abovementioned.

ARTICLE III.

From the System of M. Burnet.

Thomas Burnet. Telluris theoria sacra, orbis nostri originem & mutatationes generales quas aut jain subsit, aut olim Subitutus est complectens. Londini, 1681.

matter generally, and in a systematical manner, he was possessed of much understanding, and was a person well acquainted with the belies lettres. His work is in great reputation; and he has been criticised by some of the learned, among the rest Mr. Keil, who has geometrically demonstrated the errors of Mr. Burnet, in a treatise called "Examination of the Theory of the Earth. London, 1734, 2d edit." This Mr. Keil, has also resuted Whiston's system, but he treats the last author very different from the first, he seems even to be of his opinion in several cases; and he looks upon the tail of a comet to be a very probable cause for the deluge. But, to return to Burnet, his book is elegantly written: he knew how to paint noble images with great power, and to place mage-

magnificent scenes before our eyes. His plan is vast, but the execution is deficient for want of means, his reasoning is good, but his proofs are weak: and his confidence is so great that he causes his readers to lose it.

He begins by telling us, that before the deluge, the earth had a very different form from that which we at present see it in: it was at first a fluid mass, a chaos compounded of matter of all kinds, and of all forts of figures, the heaviest descended towards the center, and form a hard and folid body in the middle of the globe; around which the more lighter waters collected and furrounded the internal globe on every fide. The air, and all the liquors lighter than water, furmounted it and furrounded it in all its circumference. Between the orb of air, and that of water, an orb of oil and greafy liquor was formed lighter than water: but as the air was still very impure, and as it contained a very great quantity of small particles of terrestrial matter, by degrees these particles descended, and fell on the bed of oil, and formed a terrestrial orb blended with mud and oil; and this was the first habitable earth, and the first abode of man. This was an excellent foil, a light greafy earth made on purpose to yield to the first germs : the surface of the terrestrial globe was therefore at first equal, uniform, continued, without mountains, without feas, and without inequalities: but the earth remained only about fixteen centuries in this state, for the the heat of the fun by degrees drying this muddy crust, split it at first on the furface, foon after these cracks penetrated farther and increased so considerably by time, that at length they opened, in an instant the whole earth fell into pieces in the abyss of water, it continued, and thus the universe was made.

But all these masses of earth, by falling into the abys, dragged along with them a great quantity

of air; these struck against each other, divided and accumulated fo irregularly, that they left great cavities filled with air. The waters by degrees opened these cavities and in proportion as they filled them, the surface of the earth discovered itself in the highest parts: at length water alone remained in the lowest parts; that is to fay, the vast valleys which contains the sea. Thus our ocean is a part of the ancient, abyss, the rest is entered into the internal cavities with which the ocean communicates. The iflands are the small fragments, the continents are the great masses of the old crust, and as the rupture and the fall of this crust are made with confusion, it is not furprizing to find eminences, depths, plains and inequalities of all kinds on the furface of the earth.

ARTICLE IV.

From the System of John Woodward.

JOHN WOODWARD. An Essay towards the Natural History of the Earth, &c.

IT may be said of this author, that he attempted to raise an immense monument on a less solid base than the moving sand, and to build the edifice of the world on dust. For he pretends, that during the time of the deluge a total dissolution of the earth was made. The first idea which presents, aster having gone through his book, is, that this dissolution was made by the waters of the great abyse, which are dissued over the surface of the earth, and which have diluted and reduced into passe, stone, rocks, marble, &c. He pretends, that the abyse where this water was included, opened all at once at the voice of God, and dispersed over the surface

of the earth, the enormous quantity of water necessary to cover and furmount the highest mountains, and that God suspended the cause of the cohesion of bodies, which reduced all into duft, &c. He does not confider that by these suppositions, he adds other minutes to that of the universal deluge, or at least phyfical impossibilities which agree neither with the letter of the holy writ, nor with the mathematical principles of natural philosophy. But, as this author has the merit of having collected many important observations, and as he was better acquainted with the matters of which the globe is composed, than those His fystem, although badly who preceded him. conceived and badly digested, has nevertheless dazzled people feduced by the truth of fome particular circumstances, somewhat difficult on the probability of general consequences. We have therefore thought it our duty to prefent an extract from this work, in which by doing justice to the author's merit, and the exactness of his observations, we shall put the reader in a state of judging of the insufficiency of this system, and of the falfity of some of his remarks. All Woodward speaks of, having discovered by his fight, that all matters which compose the English earth, from the furface to the deepest places to which he descended, were dispersed by strata, and that in a great number of these there were shells and other marine productions: he afterwards adds, that by his correspondents and friends he was affured, that in other countries the earth is composed of the same, and that shells are found there, not only in the plains but on the highest mountains, in the deepest quarries and in an infinity of places. He perceived their ftrata to be horizontal and disposed one over the other, as matters are which are transported by the waters and deposited in form of sediment. These general remarks which are true, are followed by particular observations, by which he evidently shews.

that fossils found incorporated in the strata, are real shells and marine productions, not minerals and fingular bodies, the sport of nature, &c. To these obfervations, though partly made before him, which he has collected and proved, he adds others less exact. He afferts, that all matters of different strata are placed one on the other in the order of this specifical gravity, fo that the heaviest are lowermost, and the lightest uppermost. This general circumstance is not true; we must there stop our author, and shew him rocks which we see are on clay, sand, coal, bitumen, and which certainly are specifically heavier than all these matters: for if, in fact we found throughout theearth at first strata of bitumen. then chalk, then marl, clay, fand, stone, marble, and at last metals, so that the composition of the earth exactly followed the law of gravity, and that matters were placed in that order, there would be an appearance, that they might be all precipitated at the same time; which our author afferts with confidence, in spite of the evidence to the contrary; for without being a naturalist, we need only have our eye fight to be affured that we find heavy matters placed on lighter, and that confequently these fediments are not precipitated all at one time; but, that on the contrary, they have been brought and deposited successively by the water. As this is the foundation of his fystem, and which carries all the manifest marks of falsity with it; we shall follow it no farther than to shew how far an erroneous principle may produce false combinations and evil consequences. Every matter, saysour author, which composes our earth, from the summits of the highest mountains, to the greatest depths of mines and and quarries, are disposed by strata, according to their specifical weight; therefore, he concludes, all the matter which composes the globe, has been diffolved and precipitated at on time. But in what

manner, and in what time has it been diffolyed? In water, and at the time of the deluge: but there is not a sufficient quantity of water on the globe for this to be effected, fince there is more earth than water, and that the bottom of the fea is earth: and he tells us, there is more water than is requifite at the center of the earth, that it is only necesfary for it to ascend, to afford him the virtue of an universal dissolvent, and the quality of a preservative, for the shells which alone have not been dissolved, whereas marble and stone have: and afterwards to find the means for this water to enter into the abyss, and to make all this agree with the history of the deluge. This then is the fystem. the truth of which, the author does not the least doubt of: for when we oppose to him that water cannot diffolve marble, stone, and metals, especially in forty days, the duration of the deluge; he answers simply, that nevertheless it has happened fo; when he is asked, what the virtue of this water of the abyss was, to dissolve all the earth, and at the fame time preferve the shells? he fays, that he never pretended that this water was a dissolvent; but that it is clear, by facts, that the earth has been dissolved and the shells preserved. At length, when he is preffed, and he is evidently shewn that if he has no reason to give for these phenomena; his fystem does not explain any thing, he fays that we have only to imagine, that during the deluge the force of gravity, and the coherency of matter ceased on a sudden, and that by means of this supposition, the effort of which is very easy to conceive, we explain in a very fatisfactory manner, the diffolution of the old world. But, fay we, if the power which holds the parts of matter united has ceased, why have not the shells been dissolved as well as all the rest? Here he makes a discourse on the organisation of shells, and bones of animals, nimals, by which he pretends to prove that their texture being fibrous, and different from that of minerals, their power of coherence is of another kind: after all, we have, fays he, only to suppose, that the power of gravity and coherency did not entirely cease; but that it only has diminished sufficiently to disunite all the parts of minerals, but not those of animals. To all this we cannot be prevented from discovering, that our author was not so good a physician as naturalist, and I do not think it necessary seriously to refute opinions without foundation, especially when they have been imagined against the rules of probability, and have only been drawn from consequences contrary to mechanical laws.

ARTICLE V.

Exposition of some other System.

WE see clearly that the three forementioned hypothesis's have much in common with each other. They all agree in this point, that during the deluge, the earth changed its form, as well externally as internally: thus all thefe speculators, have not confidered that the earth before the deluge being inhabited by the same kind of men and animals, must necessarily be nearly such as it at prefent is; and that in fact, the holy writ teaches us, that before the deluge there were rivers, feas, mountains, forests, and plants, on the earth. That these rivers and mountains were for the most part the fame, fince the Tigris and Euphratis were the rivers of the ancient paradife: that the mountain of Armenia on which the ark rested, was one of the highest mountains in the world at the deluge, as it is present: that the same plants and animals which

exist now, existed then, fince the serpent, the raven, and the dove, which brought the olive branch, are fpoken of; for although Tournifort afferts there are no olive trees for more than four hundred miles from Mount Ararat, and passes some absurd jokes thereon, (Voyage du Levant, vol. 11, page 336) it nevertheless certain there were some at the time of the deluge, fince holy writ affures us of it, and it is not at all affonishing that in the space of 4000 years the olive trees have been destroyed in these quarters, and multiplied in others; it is therefore contrary to scripture, that those authors have supposed the earth was quite different from its prefent state before the deluge; and this contradiction of their hypothesis's, with the facred text, as well as their opposition to physical truths, must cause their systems to be rejected, if even they should agree with some phenomena. Burnet had neither obfervations nor any real circumstances for the foundation of his fystem. Woodward has only given us an effay, in which he promifed much more than he could perform: his book is a project, the execution of which has not been feen, We only fee, that he made use of two general observations; the first, that the earth is every where composed of matters which formerly were in a flate of foftness and fluidity, transported by the waters, and deposited in horizontal strata: the 2d, that there are marine productions in every part of the earth. To give a reafon for these circumstances, he has recourse to the universal deluge, or rather it appears, that he gives them only as proofs of the deluge, but like Burnet he falls into evident contradictions, for it is not to be fupposed with them, that there were no mountains prior to the deluge, fince it is precifely and very clearly proved, that the waters covered the highest mountains. On the other hand, it is not faid, that thefe waters deftroyed, and diffolved thefe moun-VOL. V. tains; Eee

tains; but, on the contrary, these mountains remained in their places, and the ark rested on that which the water left first visible. Besides, how can it be imagined, that during the short duration of the deluge, the waters were able to dissolve the mountains and the earth! is it not an abfurdity to fay, that in forty days the water diffolved all marble, rocks, ftones, and minerals? Is it not a manifest contradiction to admit this total diffolution, and at the fame time to fay, that the shells and marine productions were alone preserved, so that we find them at prefent entire, and the fame as they were before the deluge? I shall not fear therefore to fay, that Woodward with excellent observations has only formed a bad fystem. Whiston who came the last, greatly enriched the others, but by giving a vast scope to his imagination, has he not at least fell into contradiction? He speaks of matters not very credible, but they are neither absolutely nor evidently impossible. As we are ignorant of what is at the center and bowels of the earth, he thought he might suppose, that the internal part was occupied by a folid matter, furrounded with a heavy fluid, and afterwards with water, in which the external crust of the globe was fuftained, and in which the different parts of this crust were, more or less funk, in proportion to their relative weight or lightness, which has produced mountains and inequalities in the furface of the earth. It must be acknowledged, that this aftronomer has committed a mechanical fault; he did not dream that the earth according to this hypothesis's, must be hollow, and that consequently it could not be borne on the water it contains, and much less funk therein. I do not know but there are many other phyfical errors in his fyftem. There are a great number, as well in metaphyfics as in theology, but on the whole it cannot be denied absolutely that the earth meeting

meeting with the tail of a comet when it approached its perihelium, might not be inundated, especially allowing the author that the tail of a comet may contain aqueous vapours. It cannot be also denied, as an an absolute impossibility, that the tail of a comet, in returning from the perihelium might not burn the earth, if we suppose with the author, that the comet passed very near the sun, and was prodigiously heated during its passage; it is the same with the rest of the system: But as there is no absolute impossibility, there is so little probability to each thing taken separately, that there results an impossibility

for the whole taken together.

The three fystems we have spoken of are not the only works which have been composed on the theory of the earth; a Memori of Mr. Bourquet appeared in 1729, printed at Amsterdam, with his philosophical letters on the formation of falts, &c. in which he gives a specimen of the system he meditated, but which he has not composed, having been prevented by death. We must do justice to this author, no person has better collected phenomena and facts; to him we owe that great and beautiful observation, the correspondence of the angles of mountains. He prefents every thing which relates to thefe matters in great order, but with all those advantages, it appears that he has no better fucceeded than the rest in making a phyfical and reasonable history of the changes happened to the globe, and that he was very wide from having found the real cause of the effects he relates; to be convinced of it, we need only cast our eyes on the propofitions which he deduces from the phenomena, and which ought to serve for the basis of his theory. He fays, that the whole globe took its form at one time, and not fucceffively, that the form and disposition of the globe necessarily supposes that it has been in a state of fluidity, that the prefent state of the earth is very different from that in which it has been for many ages after its first formation; that the matter of the globe was from the beginning less dense than it has been fince it has altered its appearance; that the condenfation of the folid parts of the globe diminishes with the velocity of the globe itself, so that after having made a number of revolutions on its axis, and round the fun, it found itself on a sudden in a state of diffolution which destroyed its first structure, which happened about the vernal equinox; that in the time of this diffolution the shells introduced themfelves into the diffolved matters: that after this diffolution the earth took the form that we see it in, and that the fire directly infused itself therein, consumes it by degrees, and still continues to increase; so that it will be one day destroyed by a terrible explosion, accompanied with a general fire, which will augment the atmosphere of the globe, and will diminish its diameter, and that then the earth, instead of beds of fand or earth, will have only strata of calcined metal, or minerals, composed of amalgamas of different metals.

This is sufficient to shew the system the author meditated; to devine in this manner the past, and predict the future, and still to divine and predict nearly as others have predicted, does not appear to me to be an effort of judgment; this author also had much more knowledge and erudition, than sound and general views, and he appears to be deficient in the part necessary to physicians, of that metaphysic which collects particular ideas, which renders them more general, and which raises the mind to the point where it ought to be to see the links of causes and effects.

In the acts of Leipfic, page 40, the famous Leibnitz gave a scheme of quite a different system, under the title of *Protogea*. The earth, according to Bourquet and others, must end by sire; according

to Leibnitz it began by it, and has suffered many more changes and revolutions than is imagined. The greatest part of the terrestrial matter was furrounded by a violent fire during the time when Moses fays, light was divided from darkness. The planets, as well as the earth, were fixed stars, luminous of themselves. After having burnt a long time, he pretends that they were extinguished for want of combustible matter, and are become opaque bodies. Fire by the melting of matters has produced a vitrified crust, and the basis of all the matter which compofes the globe is glass, of which the fand is only fragments; the other kinds of earth are formed from a mixture of this fand with fixed faits and water, and when the crust cooled, the humid parts which were railed in form of vapours, refell, and formed the They at first surrounded the whole surface of the globe, and even furmounted the highest parts which at prefent forms continents and islands. According to this author, the shells and other wrecks of the fea every where found, proves that the fea has covered the whole earth; and the great quantity of fixed falts, fand, and other melted and calcined matters, which are included in the bowels of the earth, proves that the fire has been general, and that it preceded the existence of the sea. Although these thoughts are void of proofs, they are capital, and we plainly perceive them to be the production of the meditations of a great genius. The ideas have connection, the hypothefis's are not abfolutely impossible, and the consequences that may be drawn therefrom are not contradictory; but the grand defect of this theory is that it is not applicable to the present state of the earth, it is the past which it explains, and this past is so far back, and has left us fo few remains that we may fay what we please of it, and that in proportion as a man has more fancy, he may speak things which will carry. with them the air of probability. To affert, as Whiston has done, that the earth has been a comet, or to pretend with Leibnitz that it has been a fun, is faying things equally possible or impossible, and to which it would be fuperfluous to apply the rules of probability. To fay that the fea formerly covered all the earth, that it furrounded the whole globe, and that it is for this reason shells are every where found: this is not paying attention to a very effential point, which is the unity of the time of the creation; for if that was fo, it should necessarily be faid, that shell fish and other inhabitants of the sea, of which we find the remains in the internal part of the earth, existed the first, and a long while before man, and all terrestrial animals. Now independent of the testimony of holy writ, have we not reason to think, that all animals and vegetables are nearly as antient as each other?

M. Scheuchzer, in a differtation, which he has addressed to the academy of sciences in 1728, attributes, like Woodward, the change, or rather the fecond formation of the furface of the globe, to the universal deluge; and to explain that of mountains, he fays, that after the deluge, God chufing to return the waters into subterraneous refervoirs, broke and displaced with his all-powerful hand a great number of beds before horizontal, and raifed them to the furface of the globe; the whole differtation is composed to imply this opinion. As it was requisite, for these heights or eminences should be of a very solid confistence, M. Scheuchzer remarks, that God only drew them from places where there were many stones; from hence, fays he, it proceeds, that those countries, as Switzerland, where there are a great quantity, are mountainous; and on the contrary, those which, as Flanders, Germany, Hungary, and Poland, have only fand or clay, even to a very great depth, are almost entirely without mountains (See the Hist, of

the Acad. 1708, page 32.)

This author more than any other is defirous of blending physic with theology, and as he has given us some good observations, the systematical part of his works is still worse than that of those who have preceded him. On this subject he has even made declamations and ridiculous witticisms. See the complaint of the first piscium querelæ, &c. without speaking of his large work in many solio volumes, Physica Sacra, a puerile work, which appears to be composed less for men, than to amuse children, by engravings and images, which are dissured throughout without design or necessity.

Stenon, and some others after him, have attributed the cause of the inequalities of the earth to particular inundations, earthquakes, &c. but the effects of these secondary causes have been only able to produce some slight changes. We admit of these causes after the first cause, which is the motion of slux and reflux, and the motion of the sea from east to west. On the whole, Stenon nor the rest have given neither theory nor even general sacts on this matter. See the Diss. de Solo Solido infra Solidum,

&c.

Ray pretends that all mountains have been produced by earthquakes, and he has composed a treatise to prove it; we shall shew under the article of Volcano's, what little foundation his opinion is

built upon.

We cannot dispense with observing that most of the authors we have spoken of, as Burnet, Woodward, and Whiston, have committed a fault which appears to deserve to be cleared up, which is, to have looked upon the deluge as possible by the action of natural causes, whereas scripture presents it to us as produced by the immediate will of God, there is no natural cause which can produce on the whole whole furface of the earth the quantity of water required to cover the highest mountains, and if even we could imagine a cause proportionate to this effect, it would be still impossible to find another cause capable of causing the water to disappear, for by allowing Whiston, that these waters proceeded from the tail of a comet, we must deny him that it proceed from the great abyss, and that they all returned there again, fince the great abyss, according to him, being furrounded on every fide by the cruft or terrestrial orb, it is impossible that the alteration of the comet could cause to the fluids contained in the internal part of this orb, the least motion. Confequently the great abyfs will not have endured, as he fays, a violent flux and reflux; hence there will not be iffued from, nor entered therein, a fingle drop of water, and at least to suppose that the water fallen from the comet, has been destroyed by a miracle, it would still be on the furface of the earth, covering the fummits of the highest mountains. Nothing better characterizes a miracle than the impossibility of explaining the effect of it by natural causes. Our authors have made vain efforts to give a reason for the deluge; their phyfical errors on the subject of second causes, which they make use of, proves the truth of the circumstance as reported in the scriptures, and demonstrates that it could only have been performed by the first cause, the will of the Almighty.

Besides it is easy to make appear that it was neither at one, and the same time, nor by the effect of the deluge that the sea left dry the continent we inhabit: for it is certain by the testimony of holy writ, that the terrestrial paradise was in Asia, and that Asia was a continent inhabited before the deluge: consequently it was not at that time the sea covered this considerable part of the globe. The earth before the deluge therefore was nearly as it is at present, and this enormous quantity

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of water which divine justice caused to fall on the earth to punish guilty men, in fact, brought death on every creature; but it produced no change on the furface of the earth, it did not even destroy plants, fince the dove brought an olive branch to the ark.

Why therefore imagine, as most of our naturalists have done, that this water totally changed the furface of the globe, to a thousand and two thousand feet depth? Why do they defire it to be the deluge which has brought the shells on the earth which we meet with at 7 or 800 feet depth in rock and marble? Why fay, that the hills and mountains were formed at that time? And how can we figure to ourfelves that it is possible for these waters to have brought masses and banks of shells 100 miles long? I do not think any one can perfift in this opinion, at least, without admitting a double miracle in the deluge: the first, for the augmentation of the waters, and the second for the transportation of the shells; but as there is only the first which is related in the bible, I do not fee it necessary to make the fecond an article of our creed.

On the other hand, if the waters of the deluge, after having stayed above the highest mountains, afterwards retired all at once, they would have carried fo great a quantity of shells, &c. that the earth would not have been cultivable nor proper to receive trees, &c. till many ages after this inundation, as is known, by the deluge which happened in Greece, the overflown country was totally forfaken, and could not receive any cultivation for more than three ages after. See Acta erudit Lepis, Ann. 1691, page 100. We ought also to look on the universal deluge as a fuper-natural means of which the Almighty made use for the chastisement of mankind, and not as a natural effect in which all would be passed according to physical laws. The universal deluge, therefore, is a miracle in its cause and effects: we see clearly by the feripture, that it ferved for the Fff

destruction of men and animals, and that it did not in any mode change the earth, since after the retreat of the waters, the mountains and even the trees were in their places, and the surface of the earth was proper to receive culture and produce vines and fruits. How could all the race of fish which did not enter the ark, be preserved, if the earth had been dissolved in the water, or only if the waters had been sufficiently agitated to transport shells from India to

Europe, &c?

Nevertheless this supposition, that it was the univerfal deluge, which has transported the shells of the fea into every climate, is become the opinion or rather the superstition of common naturalists. Woodward, Scheushzer, and some more, call these petrified shells, the remains of the deluge; they looked on them as the medals and monuments which God left us of this terrible event; in order, that it never should be effaced from the human race: in short, they have adopted this hypothesis with so much respect, not to say absurdly, that they appear only to be employed in feeking out means to reconcile holy scripture with their opinion, and instead of making use of their observations, and derive light therefrom, they are enveloped in the clouds of a phyfical theology, the obscurity of which is derogatory to the brightness and dignity of religion, and only leaves the abfurd to perceive a ridiculous mixture of human ideas and divine deeds: to pretend, in fact, to explain the univerfal deluge, and its phyfical causes, to attempt to teach what passed in the time of that great revolution, to divine what were the effects of it, to add circumstances to those of holy writ, to draw confequences from fuch circumstances, is only an attempt to measure the power of the Most High! The wonders which his benevolent hand performs in nature in an uniform, and regular manner, are incomprehensible, and by the strongest reason, these enow by the feripture, 'that it ferved for the wonderful strokes, miracles, &c. ought to hold

us in wonder, adoration and filence.

But they will fay, the univerfal deluge being a certain fall, it is not permitted to reason on the consequences of it? All in good time, but it is requifite that you fhould begin by allowing that the univerfal deluge could not be performed by phyfical causes; you ought to know it is an immediate effect of the will of the Almighty; you ought to confine yourselves to know only what the holy writ teaches us, to own at the same time that it is not permitted you to know more, and particularly not to blend bad physical reasoning with the holy writ. These precautions, which the respect we owe to God exacts, being taken, what does there remain for examination on the subject of the deluge? Does the feripture fay mountains were formed by the deluge? No, it fays, on the contrary, is it faid, that the agitation of the waters was so great as to raise up shells from the bottom of the fea and transport them all over the earth? No, the ark floated quietly on the waves; is it faid, that the earth fuffered a total diffolution? None at all; the recital of the facred historian is simple and true, that of these naturalists complex and fabulous.

ARTICLE VII.

GEOGRAPHY.

THE surface of the earth is not like that of Jupiter, divided by bands alternative and paralell to the equator; on the contrary, it is divided from one pole to the other by two bands of earth and two of sea; the first and principal is the ancient continent, the greatest length of which is found to be in a diagonal line with the equator, and which must be measured by beginning on the north of

the most eastern part of Tartary, from thence to the land which borders on the gulph of Linchidolkin, where the Muscovites sish for whales, from thence to Tobolski, from Tobolski to the Caspian sea, from the Caspian sea to Mecca, and from Mecca to the western part of the country inhabited by the Gauls in Africa, afterwards to Moneconugi, Monomotape, and at last the Cape of Good Hope: this line, which is the greatest length of the old Continent, is about 3600 miles Paris measure: it is only interrupted by the Caspian and Red Sea, the breadths of which are not very considerable, and we must not pay any regard to these interruptions, when it is considered, as we do, that the surface of the globe is divided

only in four parts.

This greatest length, is found by measuring the continent diagonally; for, if on the contrary we measure it according to the Meridians, we shall find that there are only 2500 miles from the northern Cape of Lapland to the Cape of Good Hope, and that we cross the Baltic in its length, and the Mediteranean throughout its whole breadth, which causes a much less length, and greater interruptions than by the first road. With respect to all the other distances that might be measured in the old continent under the fame Meridans, we shall find them to be much smaller than this; having, for example, only 1800 miles from the meridional point of the island of Ceylon to the northern coast of Nova Zembla; likewise if we measure the continent paralelly to the equator, we find that the greatest uninterrupted length is found from the western coast of Africa to Zrefana, as far as Nisingpo on the eastern coast of China, and that it is about 2800 miles: that another interrupted course may be measured from the point of Brittany to Brest, as far as the coast of Chinese and Tartary about 2300 miles: that by measuring from Bergan in Norway to the coast of Kamlschaske, there is no more than 1800 miles. All these lines have, as we see much less length than the first, therefore the greatest extent of the old continent, is in fact from the eastern Cape of the most western part of Tartary, as far as the Cape of

Good Hope, that is 3600 miles.

This line, which is interrupted only by the Mexican gulph, which must be looked upon as a Mediterranean Sea, may be about 20500 miles long, and divides the new continent into two equal parts, the left of which is 1,069286 miles square on its surface, and that which is on the right is 1070926; this line which forms the middle of the band of the new continent, is inclined to the equator about 30 degrees, but in an opposite direction, so that that of the old continent extends from the north west to the south east, and all those lands together as well of the old as of the new continent, makes about 708099; miles square, which is not near, the third of the whole surface which contains 20 millions.

It must be remarked, that these two lines which cuts the continents, in their greatest lengths, and which divides each of them into two equal parts, inclines both to the same degree of southern and northern latitude. It may also be observed, that the two continents make opposite projections, which sace each other, to wit, the coasts of Africa from the Canary islands, to the coasts of Guinea, and those of America, from Guinea to the mouth of Rio

Janeira.

It appears therefore that the most ancient land of the globe is on the two sides of these lines at a moderate distance, for example, at 2 or 250 miles on each side, and by following this idea, which is founded on the observations before related, we shall find in the old continent, that the most antient lands of Africa, extend from the Cape of Good Hope to the Red Sea, and as far as Egypt, about 500 miles broad, and that consequently all the western coasts of Africa, from Guinea to the straits of Gibraltar,

raltar, are the newest lands. So likewise we shall discover that in Asia, if we follow the line on the fame breadth, the most antient lands are Arabia, Felix, and Deserta, Persia, Georgia, part of Tartary, Circaffia, and Moscovy, &c. that confequently Europe is more modern, and perhaps also China, and the eastern part of Tartary. In the new continent we shall find the Magellanic, the eastern part of Brazil, the Amaconic, Guiana, and Canada, to be the most modern lands in comparison with Peru, Terra Firma, and the islands of the gulph of Mexico, Florida, Mississippi, and Mexico; to these observations we may yet add two very remarkable circumstances; the old and new continent are almost opposite each other; the old is more extensive to the north of the equator than the fouth; on the contrary, the new is more fo to the fouth than the north of the equator; the centre of the old continent is at 16 or 18 degrees north latitude, and the centre of the new is at 16 or 18 degrees fouth latitude, fo that they feem to be made to counterbalance each other. There is also a fingular connection between the two continents, although it appears to me to be more accidental than those which I have fpoken of, which is, that the two continents would be each divided into two parts, all four of which would be furrounded by the fea, if it were not for the two fmall ifthmus's, Suez and Panama.

This is the most general idea which an attentive inspection of the globe furnishes us with, on the division of the earth. We shall abstain from forming
hypothesis's thereon and hazarding reasonings which
might lead us into false conclusions; but, as no one as
yet has considered the division of the globe to be under this point of view, I have thought it my duty to
communicate these remarks. It is very singular that
the line which forms the greatest length of the terrestrial continents, divides them into two equal parts; it

is no less so that these two lines commence and end at the fame degrees of latitude, and are both alike inclined to the equator. These relations may belong to fomething general, which may, perhaps, be difcovered, but which we yet are ignorant of. We shall fee hereafter the inequalities of the figure of the continents examined more fully; it is fufficient here to observe, that the most ancient countries are the nearest to these lines, and at the same time, the highest that also the most modern lands must be the farthest. and at the fame time the lowest. Thus in America the country of the Amazons, Guiana, and Canada. will be the most modern parts; by casting our eyes on the map of this country, we fee the waters are dispersed on every side, that there are a great number of lakes and very great rivers, which also indicates that these lands are more modern. So likewise all Africa is very mountainous, and that part of the world is very antient. There is only Egypt, Barbary, and the western coasts of Africa. As far as Senegal, which may be looked upon as modern countries. Afia is an old land, and perhaps the most antient of all, particularly, Arabia, Perfia, and Tartary. but the inequalities of this vast part of the world demands, as well as those of Europe, a detail which we refer to another article. It might be faid in general, that Europe is a new country, the tradition of the migration of the people, and of the origin of arts and sciences, appears to indicate it: it is not long fince it was filled with morasses, and covered with forests, whereas in the land very antiently inhabited, there is but little wood, little water, no moraffes, much land, and a great quantity of mountains, whole fummits are dry and sterile, for men destroy the woods, conftrain the waters, confine rivers, dry up moraffes, and in time give a different face to the earth, from that of inhabited or newly-peopled countries. A one on a she all the fand on the fire fire

The antients were acquainted but with a fmall part of the globe. All America, Magellanic, and a great part of the internal part of Africa, was entirely unknown to them. They only knew that the torrid zone was inhabited, altho' they had navigated around Africa; for it is 2200 years fince the new King of Egypt gave vessels to the Phenicians which departed from the Red Sea, coasted round Africa, doubled the Cape of Good Hope, and having employed two years in this voyage, the third year they entered the straits of Gibrastar, (Vide Herodotus, lib. iv.) Nevertheless the antients were not acquainted with the property the load stone had, if turning towards the poles, although they knew that it attracted iron. They were ignorant of the general cause of the flux and reflux of the sea, they were not certain the ocean furrounded the globe without interruption; fome indeed suspected it, but with so little foundation, that no one dared to fay or even conjecture it was possible to make a voyage round the world. Magellan was the first who made it A. D. 1519 in 1124 days. Sir Francis Drake was the fecond in 1577, and he did it in 1056 days; afterwards Thomas Cavendis made this great voyage in 777 days, in the year 1586. These famous travellers were the first who demonstrated physically, the sphericity and the extent of the earth's circumference: for the ancients were very wide from having a just measure of this circumference, although they had travelled a great deal. The general and regulated winds, and the use to be made of them in long voyages, were also absolutely unknown to them; therefore, we must not be furprized at the little progress they made in Geography, fince at prefent, in spite of all the knowledge we have acquired by the aid of Mathematical Sciences, and the discovery of navigators, many things remain still to be found, and vast countries to be discovered. Almost all the land on the side of the Atlantic pole is unknown to us; we only know that there is some, and that it is separated from all the other continents by the ocean. Much land also remains to be discovered on the side of the Arctic pole, and we are obliged to own with some kind of regret, that for more than a century the ardor of discovering new countries is extremely abated. We have preferred, and that with reason, the utility of making those we are acquainted with of value to us,

to the glory of conquering new.

therto make a feparate world.

Nevertheless, the discovery of these northern countries would be a great object of curiosity, and might be useful. We have discovered on that side only some coasts, and it is vexatious that navigators, who have chosen to attempt this discovery, at different times, have almost always been stopt by the ice. The sogs, which are very considerable in those latitudes, is another obstacle, yet in spite of these inconveniencies, it is to be thought that by sailing from the Cape of Good Hope at different seasons, we might at last discover a part of these lands, which his

as the ice and fogs have stopt all the navigators who have undertaken the discovery of the northern countries, by the Atlantic, and that the ice presented itself in the summer at these climates, as well as in other seasons, might we not promise ourselves better success, by changing our rout? I imagine we might attempt to arrive at these countries by the Pacisic Sea. sailing from Baldivia or any other part on the coast of Chili, and traversing this sea under the 50th degree south latitude; there is not the least appearance that this navigation, which has never yet been made, is perilous, and it is probable we should find by this method, new countries; for what remains for us to

There is also another mode which might succeed,

rable, that we may without deceiving ourselves, esti-Vol. V. Ggg mate

know on the coast of the northern pole, is so conside-

mate it at more than a quarter of the superficies of the globe, that there may be in those climates a terrestrial continent as large as Europe, Asia, and Afri-

ca, all taken together.

As we are not at all acquainted with this part of the globe, we cannot juftly know the proportion between the furface of the earth, and that of the fea; only as much as may be judged by infpection of what is

known, there is more fea than land.

If we would have an idea of the enormous quantity of water which the fea contains, let us suppose one common and general depth to the ocean, by computing it only at 200 fathom or the 10th part of a mile, we shall fee that there is sufficient water to cover the whole globe to the height of 600 feet of water, and if we would reduce this water into one mass, we shall find that it forms a globe of more than 60 miles diameter.

Navigators pretend, that the continent of the northern countries is much colder than that of the arctic pole, but there is no appearance that this opinion is founded on truth, and probably it has not been adopted by travellers, because they have found ice in a latitude where it is scarcely ever found in our fouthern feas, and that may proceed from some particular causes. We find more ice in April above 67 and 68 degrees, northern latitude; and the favages of Arcadia and Canada fay, when it is not all melted in that month, it is a fign the rest of the year will be cold and rainy. In 1725 there may be faid to have been no fummer, and it rained almost continually, as also not only the ice of the northern seas was not melted in April in the 67th degree, but even it was found the 15th of June towards the 4th and 42d degree. (See the Hist. of the Acad. Ann. 1725.)

We find a great quantity of this floating ice in the northern fea, especially at some distance from land. They came from the Tartarian fea into that of Nova

Zembla, and other parts of the frozen fea. I have been affured by people of credit, that an English Captain, named Monfon, instead of feeking a paffage between the northern land to go to China, directed his courfe strait to the pole, and had approached it within two degrees; that in this course he had found a high fea, without any ice, which proves that the ice is formed near land, and never in open fea; for if even we should suppose, against all appearance, that it might be cold enough at the pole to freeze over the furface of the fea, we cannot conceive better how these enormous mountains of ice which float, could be formed, if they did not find a fixed point against land, from whence afterwards they were loofened by the heat of the fun. The two veffels which the East-India company fent in 1739, to discover the northern land, found ice in the latitude of 46 and 48 degrees, but this ice was not very far from land, fince they discovered it, without being able to land there. This we imagine must proceed from the internal and adjoining lands of the northern pole, and it may be conjectured that they followed the course of many great rivers, which water the unknown land, the fame as the Oby, Jenisca, and other great floods, which fall into the northern feas, carry with them the ice, which, during the greatest part of the year, stops up the strait of Waigats, and renders the Tartarian fea unnavigable by this courfe, whereas beyond Nova Zembla, and nearer the poles, where there are few rivers, and but little land. Ice is not fo frequently met with, and the fea more navigable, so that if they would ftill attempt the voyage to China and Japan, by the northern feas, we should possibly, to get away from the land and ice, thape our courfe to the pole, and feek the highest seas, where certainly there is but little or none, for it is known that falt water can, without freezing, become colder than foft water frozen, and confequently the excessive cold of the pole may possibly render the sea colder than the ice, without the surface being frozen, so much the more as at 80 or 82 degrees, the surface of the sea, although mixed with much snow and soft water, is only frozen near the shore. By collecting the testimonies of travellers on the passage from Europe to China by the northern sea, it appears that it does exist, and that if it has been so often uselessly attempted, it is because we have always seared to go far from land, and ap-

proach the pole.

Captain William Barents who failed, as well as others, in his northern voyage, yet did not doubt but that there was a paffage, and that if he had gone farther from shore, he had found an open sea free from ice. Muscovite travellers fent by the Czar to discover the northern seas, relate that Nova Zembla is not an ifland, but belonging to the continent of Tartary, and that to the north of Nova Zembla, is a free and open sea. A Dutch traveller afferts, that the fea throws up whales, on the coasts of Corea and Japan, which have English and Dutch harpoons on their backs. Another Dutchman has pretended to have been even under the pole, and afferts it is as warm there as it is at Ansferdam in the middle of An Englishman named Golding, who made more than 30 voyages to Greenland, related to King Charles II. that two Dutch veffels with which he had failed, having found no whales on the coast of the island of Edges, resolved to proceed farther north, and that being returned at the expiration of fifteen days, these Dutchmen told him, that they had been as far as 89 degrees latitude, i. e. within one degree of the pole, and that they found no ice there, buta free and open fea, very deep, and like that of the bay of Biscay, and that they shewed him four jorunals of two veffels, which attested the same thing, and

and agreed very near with what they affirmed. In short, it is related in the Phil Trans, that two navigators, who had undertaken the discovery of this passage, shaped a course 300 miles eastward of Nova Zembla, but that the East-India company, whose interest it was that this passage should not be discovered, hindered those travellers from returning. (See the collection of northern voyages, page 200), But the Dutch East-India company thought on the contrary that it was their interest to find this passage; having attempted it in vain on the fide of Europe, they fought it by that of Japan, and they would apparently have fucceeded. if the Emperor of Japan had not forbidden all navigation on the fide of the land of Jeffo. This passage therefore cannot be found but by failing to the pole beyond Spitzberg, or by following the high fea between Nova Zembla and Spitsberg, under the 72d degree latitude, If this sea has a confiderable breadth, we need not fear to find it frozen at this latitude, nor even under the pole, for reasons we have alledged; in fact, there is no example, that the furface of the fea has been found frozen to a confiderable distance from the shore; the only example of a fea being frozen entirely over, is that of the Black Sea, it is narrow, and somewhat falt, and receives a very great number of rivers, which come from the northern countries, and which bring ice there; it freezes therefore fometimes fo violent, that its furface is frozen over, even to a confiderable depth, and if we credit historians, it froze in the time of the Emperor Copronymus thirty fathom deep, without reckoning twenty fathom of snow which was on the ice. This matter appears to be exaggerated, but it is certain, that it freezes over almost every winter; whereas the high feas, which are a thousand miles nearer the pole, do not freeze at all, which can only proceed from the faltness, and the little ice it receives by the rivers, in comparison of the enormous quantity which they transport into the Black Sea.

This ice, which is looked upon as a barrier which opposes the navigation towards the poles, and the discovery of the northern continent, proves only, that there are very great rivers, adjoining to the climates where it is met with; confequently it indicates to us also, that there are vast continents from whence these rivers flow; nor must we be difcouraged at the fight of these obstacles; for if we confider we shall easily perceive, that this ice must be only in some particular places; that it is almost impossible that in the whole circle which may be imagined terminates the northern continent on the fide of the equator, there is throughout great rivers, on which ice floats, and confequently there is a great appearance that we should succeed, by directing our course towards some other point of this circle. Befides the description which Dampier and some other travellers have given us of the soil of New Holland, makes us suspect that this part of the globe, which borders on the northern lands, and which perhaps makes a part of it, is a country less antient than the rest of this unknown continent: New Holland is a low country without water, without mountains, but thinly inhabited, and where the natives are favages, and without industry; all this concurs to make us think that they are in this continent nearly what the favages of Amaconia or Paraguai are in America. We have found polifhed men, empires, and kings, at Peru and Mexico, i. e. in the highest, and consequently the most antient countries of America. The favages on the contrary are found in the lowest and most modern countries, therefore we may prefume that on the inland parts of the northern countries, we should also find men united in fociety in the upper countries, from whence these great rivers, which brings this prodigious ice to the sea, derives their source.

The internal part of Africa is unknown to us, almost as much as it was to the ancients. They had like us, made the tour of that almost-island by sea, but in fact, they have left us neither charts nor defcriptions of the coasts. Pliny informs us, that the tour of Africa had been made fince the time of Alexander, that the wrecks of Spanish vessels had been discovered in the Arabian sea, and that Hanno, a Carthaginian general, had made a voyage from Gades to the Arabian fea, and that he had even written a relation of this voyage. Befides that, he fays, Cornelius Nepos tells us, that in his time a certain Eudoxus, perfecuted by the king Lathurus, was obliged to fly from his country; that departing from the Arabian gulph, he arrived at Gades, and that before this time they traded from Spain to Ethiopia by fea, (Vide Pliny, Hist. Nat. vol. 1. lib. 2) Nevertheless, in spite of these testimonies of the ancients, we are perfuaded that they never did double the Cape of Good Hope, and the course which the Portuguese took the first to go to the East-Indies, was looked upon as a new discovery; it will not perhaps therefore be deemed amis to the belief of the 9th century.

"In our time an entire new discovery has been made, which was wholly unknown to those who lived before us. No one thought that the sea, which extends from India to China, had a communication with the Syrian sea, nor could they be brought to conceive it. Let us see what has happened in our time, according to what we have learnt. We have found in the sea Roum or Mediterranean, the wreck of an Arabian vessel, shattered by the tempest, and all the crew perished. The waves had broken it to pieces, and they were carried by the wind and waves to the Cozar sea, and from thence

thence to the Mediterranean, from whence they were at length thrown on the coast of Syria. This shews that the sea surrounds China and Cila, the extremity of Turquestan, and the country of the Cozars; that it afterwards flows by the strait till it has washed the coast of Syria. The proof is drawn from the construction of the vessel we speak of, for no other vessels but those of Siraf being built without nails, but joined together in a particular manner, like as if they were sewed, whereas those of all the vessels of the Mediterranean and of the coast of Syria are nailed and not joined in this manner (see the ancient relations of travels by land to China, p. 53, and 54.)

To this the translator of this ancient relation adds, Abuziel remarks as a new and very extraordinary thing, that a veffel was carried from the Indian fea, on the coasts of Syria. To find a passage into the Mediterranean: he supposes there is a great extent above China, which has a communication with the Cozar fea, that is, with Mufcovy. The fea which is below the Cape of Currents was entirely unknown to the Arabs, by reason of the extreme danger of the navigation, and the continent was inhabited by fuch barbarous people, that it was not eafy to fubject them nor even to civilize them by commerce. From the Cape of Good Hope to Soffala the Portuguese found on established Moors, as they have fince found in all the maritime towns as far as China, this town was the last known to geographers, but they could not tell whether the sea, by the extremity of Africa, had a communication with the fea of Barbary, and they contented themselves with describing it as far as the coast of Zinge. This is the reason why we cannot doubt but that the first discovery of the passage of this sea by the Cape of Good Hope, was made by the Europeans under the conduct of Vasco de Gama, or at least some years before he doubled the cape, if it is true that there are marine charts older than this this navigation where the Cape is called by the name of Frontiera du Africa. Antonio Gulvan testifies from the relation of Francisco de Sousa Tavares, that in 1528, the infant Don Fernand, shewed him such a chart, which he found in the monastery of Acoboca, and which had been made 120 years, perhaps from that faid to be in the treafury of St. Mark at Venice, and which is thought to have been copied from that of Mark Paolo, which also marks the point of Africa according to the testimony of Ramusio, &c." The ignorance of those ages on the subject of the navigation around Africa, will appear perhaps less singular than the silence of the editor of this ancient relation, on the subject of the paffages of Herodotus, Pliny, &c. which we have quoted, and which proves the ancients had made the tour of Africa.

Be it as it may, the African coasts are actually very well known to us, but whatever attempts have been made to penetrate into the inner parts of the country, we have not been able to attain sufficient knowledge of it to give exact relations. It might be nevertheless greatly to be wished, that we might by Senegal or some other river get farther up the country and establish ourselves there; then we should find, according to all appearances, a country as rich in precious mines as Peru or the Brasils; for it is known that the African rivers abound with much gold, and as this country is very mountainous and situated under the equator, it is not to be doubted, but it contains, as well as America, mines of heavy metals, and of the most compact and hard stones.

The vast extent of northern and eastern Tartary has only been discovered in these latter times. If the Muscovite maps are just, we are at present acquainted with the coasts of all this part of Asia, and it appears that from the point of eastern Tartary to north America, there is scarcely more than a space of four or

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five hundred miles: it has even been pretended that this tract was much shorter, for in the Amsterdam gazette of the 24th of January 1747, it is faid, under the article of Petersburg, that Mr. Stoller had discovered one of these American islands beyond Kamtschatka, and he has demonstrated, that we might go thither from Ruffia by a small tract. The jesuits and other miffionaries have also pretended to have discovered the savages in Tartary, which they had catechifed in America, which should in fact suppose that the tract is still shorter (see the hist. of New France by the Pere Charlevoix, Vol. III. p. 30 and 31.) This author even pretends, that the two continents of the old and new world join by the north, and fays, that the last navigations of the Japanese affords room to judge, that the tract of which we have spoken, is only a bay, above which we may pass by land from Asia to America. But this requires confirmation, for hitherto it has been thought with fome kind of probability, that the continent of the arcti pole is entirely seperated from the other continents, as well as that of the atlantic.

Aftronomy and navigation are carried to fo high a pitch of perfection, that it may reasonably be expected one day or other to have an exact knowledge of the whole surface of the globe. The ancients knew only a small part of it, because they had not the compass, and dared not venture into the high feas. I know well, that some people have pretended that the Arabs invented the compass and used it a long time before we did to travel on the Indian fea, and travel within China; but this opinion has always appeared destitute of all probability; for there is no word in the Arab, Turkish or Persian languages, which fignifies the compass; they make use of the Italian word Boffola: they do not even at prefent know how to make a compass, nor give the magnetical quality to the needle, but purchase them

from the Europeans. What father Maritini favs on the fubject of this invention, appears scarcely better founded; he pretends that the Chinese have been acquainted with the compass for upwards of 300 years; but if that was the case, how comes it that they made little use of it? Why did they in their voyages to Cochinchina take a course much longer than was neceffary? and why did they always confine themselves to the same voyages, the greatest of which were to Java and Sumatra? and why did not they discover before the Europeans, an infinity of fertile islands bordering on them, if they had possessed the art of navigation in the open sea? for a few years before the discovery of this wonderful property of the load stone, the Portuguese made a very great voyage; they doubled the Cape of Good Hope, they traverfed the African and Indian sea, and while they directed all their views east and fouth, Christopher Columbus turned his towards the west.

By a little confideration it was easy to devine, there were immense spaces towards the west : for, by comparing the known part of the globe, for example, the distance of Spain to China, and considering the revolution of the earth or heaven, it was easy to fee that there remained a much greater extent towards the west to be discovered, than that they were acquainted with towards the east. It is therefore not from the defect of astronomical knowledge, that the ancients did not find the new world, but only for want of the compass. The passages of Plato and Aristotle, where they speak of countries far distant from the pillars of Hercules, feems to indicate, that some navigators had been driven by tempest as far as America, from whence they returned with much difficulty, and it may be conjectured, that if even the ancients had been persuaded of the existence of this continent from the relation of those navigators, they would not have even thought it possible to strike out the road, having no guide nor any knowledge of

the compass.

I own, that it is not impossible to travel in the high feas without a compass, and that very resolute people might have undertaken to feek after the new world, by conducting themselves simply by the stars bordering on the pole. The Aftro Labe particularly being known to the ancients, it might strike them to leave France or Spain, and shape their course towards the west by keeping the polar star always to the right, and often taking the altitude to guide themselves nearly under the same parallel: it is without doubt in this manner that the Carthaginians of whom Aristotle makes mention, found the means of returning from these remote countries, by keeping the polar star to the left, but it must be allowed that a like voyage would be looked upon only as a rash enterprize, and that consequently we must not be aftonished that the ancients have not even conceived the project.

In the time of Christopher Columbus, the Acores, the Canaries, and Madeira were already discovered, It was remarked, that when the west winds lasted a long time, the fea brought pieces of foreign wood on the coasts of these islands, canes of unknown species, and even dead bodies, which by many marks were discovered to be neither European nor African. Columbus himself remarked, that on the fide of the west, certain winds blew which only remained a few days, and which he was perfuaded were land winds: nevertheless although he had all these advantages over the antients, and the knowledge of the compass, the difficulties still to conquer were so great, that there was only the fuccess he met with which could justify the enterprise; for let us for a moment suppose, that the continent of the new world had been farther distant; for example, 1000 or 1500 miles farther than it in fact is, a thing

which

which Columbus could neither know nor forfee, he would not have arrived there, and perhaps this great country would be still unknown. This conjecture is so much the better founded, as Columbus, although the most able navigator of his time, was feized with fear and aftonishment in his fecond voyage to the new world; for as at first he only found some islands, he directed his course more to the fouth, to discover other land, and was stopt by currents, the confiderable extent and direction of which always opposed his course, and obliged him to return to discover land to the west; he imagined that what had hindered him from advancing on the fouthern fide was not currents, but that the fea flowed by raising itself towards the heavens, and that perhaps both one and the other touched on the fouthern fide. True is it, that in too great enterprizes the least unfortunate circumstance may turn our brain, and abate our courage.

ARTICLE VII.

On the production of the strata, or beds of earth.

WE have before shewn in the first article, that by virtue of the mutual attraction between the parts of matter, and by virtue of the centrifugal force which results from the rotation on its axis: the earth has necessarily taken the form of a spheroid, the diameter of which differs 1-230th part, and that it can only proceed from the changes arrived at the surface, and caused by the motion of the air and water, that this difference has become greater, as is pretended to be concluded from the measures taken at the equator, and polar circle. This theory of the earth, which so well agrees with hydrostatical laws, and with our theory supposes the globe

globe to have been in a state of liquefaction, when it received its form, and we have proved that the motion of projection and relation have been imprinted at the same time by a like impulsion. We shall easily persuade ourselves that the earth has been in a state of liquefaction produced by fire, when we consider the nature of the matters which the globe incloses, the greatest part of which, as sand and clay, are vitrified or vitristable matters, and when on the other hand we restect on the impossibility there is that the earth has ever been in a state of sluidity, produced by the waters, since there is infinitely more earth than water, and that in other respects the water has not the power of dissolving stone, sand, and other matters of which the earth is composed.

I perceive then that the earth could take its figure only in the time when it was liquified by fire, and by pursuing our hypothesis, I conceive, that when the fun quitted it, the earth had no other form than that of a torrent of melted matter and inflamed vapours; that this torrent collected itself by a mutual attraction of the parts, and became a globe, to which the rotative motion gave the figure of a spheroid, and when the earth was cooled, the vapours which were first extended like the tails of comets, by degrees condensed or deposited a slime or mud mixed with sulphurous and faline matters, a part of which by the motion of the waters was swept into the perpendicular cracks where it produced metals and materials, while the rest remained on the surface, and produced that reddish earth, which forms the first strata; and which, according to different places is more or less blended with animal or vegetable particles, reduced into minute molecules, in which the organisation is no longer perceptible.

Therefore, in the first state of the earth the globe was internally composed of a vitrified matter, as I think

it at present is, above which are the parts which the fire has most divided, as sand, which are only fragments of glass, and above these sands, the lighter parts, as pumice stone, and the scoria of the vitrified matter, which have swam at top, and formed clays, &c. the whole was covered with a bed of water, 5 or 600 feet thick, produced by the condensation of the vapours when the globe began to cool. This water every where deposited a muddy bed mixed with matters which sublime and exhale, by the fire; and the air was formed of the most subtle vapours which disengaged themselves from the waters by their lightness, and surmounted them.

Such was the state of the globe when the action of the flux and reflux, the winds, and the heat of the fun, began to change the furface of the earth. The diurnal motion, and the flux and reflux, at first raised the waters under the southern climates; these waters carried with them mud, clay, and fand, and by raifing the parts of the equator, they by degrees perhaps lowered those of the poles, with this difference of about two miles, which we have mentioned, for the waters foon broke and reduced into powder the pumice stone and other spongeous parts of the vitrified matter which were at the furface; they hollowed depths, and raised up eminences which in course of time became continents, and they produced all the inequalities which we remark in the furface of the earth, and which are more confiderable towards the equator than elsewhere; for the highest mountains are between the tropics and the temperate zones, and the lowest are on the polar circle; between the tropics we have the Cordileres, and almost all the mountains of Mexico and Brazil, and Africa; to wit, the great and little Atlas, the mountains of the moon, &c. and that befides the land which is between the tropics pics, is the most unequal of all the globe, as is the sea, since between the tropics there are found many more islands than elsewhere, which evidently shews that the greatest inequalities of land is to be found in

the vicinity of the equator.

However independent my theory may be of that hypothesis of what past at the time of the first state of the globe, I have been very glad to return to it in this article, in order to flew the connection and poffibility of the fystem which I have proposed, and of which I have given the precise account in the first It must only be remarked that my theory, which forms the text of this work, does not stray far from it, as I take the earth in a state nearly similar to that we fee it in, and as I do not make use of any of the suppositions which are obliged to be used when we would reason on the past state of the terrestrial globe. But as I here present a new idea on the subject of the mud of the waters, which, according to my opinion, has formed the first bed of earth which furrounds the globes, it appears to me also necessary to give the reasons on which I found this opinion. The vapours which rife in the air, produce rain, dew, aerial fires, thunder and other These meteors are therefore blended with aqueous, aerial, fulphurous and terrestrial particles, &c. and it is these solid and terrestrial particles which forms the mud or dirt we speak of. When rain water is suffered to rest, a sediment is formed at bottom; where after having collected a quantity of dirt, if it is fuffered to stand and corrupt; it produces a kind of mud which falls to the bottom of the This mud is even in great plenty, and the dew produces much more than rain water. It is greafy, unctious and reddish substance.

The first strata which covers the earth is composed of this mud mixed with perished vegetable or animal parts, or stony and sandy particles. We may remark almost every where, that the cultivable land is reddish and more or less mixed with these different matters; the particles of fand or stone found there, are of two kinds, the one coarfe and massive, and the other finer and fometimes impalpable; the largest comes from the lower strata from which they are loofened in cultivating the earth, or the upper mould, penetrating into the lower which is of fand or other divided matters, and forms those earths we call (fat and unctious) the other stony parts which are finer, proceed from the air, fall like dew and rain, and mix intimately with the foil. This is properly the refidue of the powder which the air tranfports, and which the wind continually raifes from the furface of the earth, and which fall again after having imbibed the humidity of the air. When the earth predominates, and the stony and fandy parts are but few, the earth is then reddish and fertile : if at the fame time it is mixed with a confiderable quantity of perished animals or vegetables: the earth is blackish, and often more fertile than the first, but if the mould is only in a small quantity, as well as the animal or vegetable parts, and the earth is white and sterile; and when the fandy, stony or cretaceous parts which compose these sterile lands, are mixed with a fufficient quantity of perished animal or vegetable parts, they form the black and lighter earths which have but little fertility; fo that, according to the different combinations of these three different matters, mould parts of animals and vegetables, and particles of fand and stone, the land is more or less fecund and differently coloured.

To fix ideas, let us take the first soil which prefents itself, and which has been dug deep enough; for example, the earth of Marly-la-ville, where the pits are very deep: it is a high country, but flat and fertile, the strata of the earth of which are arranged horizontally. I had samples brought me of all these

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strata which M. Dalibard, an able botanist, versed in different sciences, had dug under his inspection; and after having tried all these matters in aqua fortis, I formed the following table of them.

The State of the different Beds of Earth found at Marly-la-Ville, at the depth of 100 Feet.

T.

F	eet In	ch.
A free reddish earth, mixed with much dirt, a very small quantity of vitrifiable fand		
II.	13	0
A free earth or foil mixed with more gravel, and a little more vitrifiable fand III.	2	6
Dirt mixed with vitrifiable fand in a very great quantity, and which made but very lit-		
tle effervescence with aqua fortis IV.	3	0
Hard marl, which made a very great effervescence with aqua fortis	2	0
Pretty hard marly stone VI.	4	0
Marl in powder, mixed with vitrifiable		
fand VII.	5	0
Very fine vitrifiable fand VIII.	1	6
Marl in earth, mixed with a little vitrifia-		
ble fand IX.	3	6
Hard marl, in which was real flint X.	3	6
Gravel, or powdered marl	r Egla	o n-
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XI.

XI.		
Feet	Incl	h.
Eglantine, a stone, of the grain and hardness of marble and sonorous	1	6
XII.	V	•
XIII.	1	6
Marble in hard stone, whose grain was very		
fine : : XIV.	1	6
Marl in stone, whose grain was not so fine XV.	1	6
More grain'd and thicker marl XVI.	2	6
Very fine vitrifiable fand, mixed with fea fossil shells, which had no adherence with		
the fand, and whose colours was perfect - XVII.	1	6
Very fmall gravel or fine marl powder - XVIII,	2	0
Marl in hard flone XIX.	3	6
Very large powdered marl XX.	1	6
Hard and calcinable ftone like marble - XXI.	1	6
Grey and vitrifiable fand mixed with fosfil shells, particularly oysters and muscles,		
which have no adherence with fand and		
which are not putrified	3	0
XXII.		
White vitrifiable fand mixed with shells - XXIII.	2	0
Sand flreaked red and white, vitrifiable		
and mixed with the like shells	1	0
XIV.		
Larger fand, but still vitrisiable and mixed		WH
with the like shells	I	
	X	XV.

XXV.

$\mathbf{A}\mathbf{A}\mathbf{V}$.			
A less to be and a constant of the control of the	Feet Inch.		
Grey, fine and vitrifiable fand mixed wit	h	8-25	
the like shells XXVI.		8	6
[12] [12] [13] [14] [15] [15] [15] [15] [15] [15] [15] [15			
Very fine fat fand, where there were only	a	-	85
few shells XXVII.	-	3	0
Gres XXVIII.		3	0
Vitrifiable fand, streaked red and white XXIX.	•	4	0
White vitrifiable fand XXX.	•	3	6
Reddish vitrisiabie sand	-	15	0
Total depths when they left off digging		101	fr.

I have before faid that I tried all these matters in aqua fortis; because where the inspection and comparison of matters with others that we are acquainted with, is not sufficient to permit us denominate and range them in the class to which they belong, and that we are troubled to decide by observation alone, there is no means more ready, nor perhaps more sure, than to tryby aqua fortis, the terrestrial or lapidistic matter: those which acid spirits dissolve immediately with heat and ebullition, are generally calcinable, those on the contrary, which resist those spirits, and on which they make no impression are vitristable.

By this enumeration we perceive, that the foil of Marly-la-Ville was formerly the bottom of a fea, which has been raifed above 75 feet, fince we find shells at that depth. Those shells have been transported by the motion of the water, at the same time as the sand in which they are met with, and the whole is sallen in form of a sediment which is arranged in a level manner, and which have produced different

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strata of grey and white sand, and sometimes also streaked with red and white, &c. the total thickness of which is sisteen or eighteen seet; all the other upper strata to the first, have been transported after the same manner by the motion of the water, and deposited in form of a sediment, which we cannot doubt, as well by reason of the horizontal situation of the strata as of the different beds of sand mixed with shells and marl, which are only the wrecks or rather the fragments of shells. The last strata itself has been formed almost entirely by the mould we have spoken of, which is mixed with a part of the marl which was at the surface.

I have chosen this example as the most disadvanta. geous for a explanation, because it at first appears very difficult to conceive, that the dust of the air, rain and dew, could produce a strata of free earth three feet thick; but it must at first be observed, that it is very rare to find, especially in land a little raifed, so considerable a thickness of cultivable earth: it is generally about three or four feet, and often not a foot thick. In places furrounded with hills, this thickness of good earth is greater, because the rain loofens the earth of these hills, and carries it into the vallies, but by here supposing nothing at all of that, I find that the last strata formed by the waters, are very thick beds of marl. It is natural to imagine, that this marl had at the beginning a ftill greater thickness, and that of the thirteen feet which compofes the thickness of the upper strata, there were many of marl, when the fea quitted the land and left it naked. This marl exposed to the air, melted with the rain, the action of the air and heat of the fun produced flaws, small chinks and altered it by all these external causes so far as to become a matter divided and reduced into powder on the furface, as we fee the marl, which we take from the quarry fall into powder when exposed to the air: the fea did not quit this land fo precipitately fince as it fometimes has covered it, whether by the alternative motion of the tides, or by the extraordinary elevation of the waters in foul weather, when it mixed with this bed of marl and other matters; when the land was at length raifed above the waters, plants began to grow there, and it was then that the dust in the rain or dew by degrees coloured and penetrated this earth, and gave it the first degree of fertility which mankind afterwards soon augmented by culture, by digging and dividing its surface, and thus giving to the dust in the dew or rain the facility of more deeply penetrating it, which at last produced that bed of free earth thirteen feet thick.

We shall not here examine whether the reddish colour of vegetable earths, proceeds from the iron which is contained therein, as it has already been taken notice of in our discourse of minerals; it is sufficient to have explained our conception of the formation of the superficial strata of the earth, and by other examples we shall prove, that the formation of the internal strata, can only be the work of the

The furface of the globe, fays Woodward, this external strata on which men and animals walk, which ferves as a magazine for the formation of vegetables, and animals, is for the greatest part composed of vegetable or animal matter, which is in All animals and vecontinual motion and variation. getables, which have existed from the creation of the world, have ever successively extracted from this frata the matter which composes them, and have after their death restored to it this borrowed matter; it remains there always ready to be retaken, and to ferve for the formation of other bodies of the fame kinds fuccessively, without even discontinuing, for the matter which composes a body is proper and natural to form another body of that kind. In inhabited

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countries, in places where wood is not cut; where animals do not brouze on the plants, this strata of vegetable earth increases very confiderably with time: in all wood, and even in those which are cut, there is a bed of mould, of fix or eight inches thick, which has been formed only by the leaves, fmall branches, and bark which perished. I have often observed on the ancient Roman way, which croffes Burgundy in a long extent of foil; that there is formed a bed of black earth more than a foot thick, which actually nourishes very high trees, and this firata is composed only of a black mould formed by the leaves, bark, and perished wood: as vegetables inhale for their nutriment much more air and water than earth, it happens, that by perishing they return to the earth more than they have taken from it; befides a forest confines the rain water, by stopping the vapours: fo in a wood which is preferved a long time without being cut, the ftrata of earth which ferves for vegetation increases confiderably; but animals restoring less to the earth than they take from it, and men making enormous confumption of wood and plants for fire and other uses, it follows that the strata of an inhabited country must diminish, and become at length like the foil of Arabia, Petrea, and like that of many other eastern provinces, which in fact are the most ancient inhabited countries, where only fand and falt is to be met with, for the fixed falt of plants and animals remains, whereas all the other parts volatilize.

After having spoken of this strata of external earth which we cultivate; let us examine the position and formation of the internal strata; the earth, says Woodward, appears in some places that are dry, composed of strata placed one on the other, as so many sediments which necessarily fell to the bottom of the water: the deepest strata are generally the thickest, and those above the thinnest, and so on to the sur-

face. We find fea shells, teeth, and bones of fish, in these different strata, they are not only found in the foft beds, as chalk, mud, &c. but even in the more folid and hard strata, as in stone, marble, &c. these marine productions are incorporated with the stone, and when broken, and the shell separated from them, we always observe the stone has received the impression with so much exactness, that we see all the parts were exactly contiguous, and applied to the shell. "I am affured, fays this author, that in France, Flanders, Holland, Spain, Italy, Germany. Denmark, Norway, and Sweden, stone and other terrestrial substances are disposed in strata as in England; that these strata are divided by parallel strokes; that there are within stones and other terrestrial and compact fubstances, a great quantity of shells andother productions of the fea, disposed in the same manner as in this island. I am informed that these strata are found the same in Barbary, Egypt, Guinea, and in other parts of Africa, in Arabia, Syria, Persia, Malabar, China, and the rest of the provinces of Asia, in Jamaica, Barbadoes, Virginia, New-England, Brafil, and other parts of America." (Essay on the natural History of the Earth, page 4, 41, 42, &c.)

This author does not fay how he learnt, or by whom he was told, that the earth of Peru contained shells, yet as in general his observations are exact, I do not doubt but he was well informed; and this persuades me, that shells may be found in the earth of Peru, as well as elsewhere. I mention this remark, from the occasion of a doubt formed some time hence on that, and of which I shall presently

appeal.

In a trench made at Amsterdam to make a pit, the earth was dry to the depth of 230 feet, and the strata of earth was found as follows: 7 feet of vegetable or garden earth, 9 feet turf, 9 feet fost clay, 8 feet fand, 4 feet earth, 10 feet fand, on which it is customary to fix the piles which support the houses of Amsterdam; then 2 feet argile, 4 of white sand, 5 of dry earth, 1 of soft earth, 14 of gravel, 8 argile, mixed with earth; 4 of gravel, mixed with shells; then clay 102 feet thick, and at last 31 feet fand, at which depth they ceased digging. (See

Varenii, Geograph. General, page 46.)

It is very rare to dig fo deep without meeting with water, and this circumstance is remarkable in many particulars, 1. It shews, that the water of the sea does not communicate with the internal part of the earth, by means of fillration or stillation, as vulgarly supposed. 2. We fee that shells are found at the depth of 100 feet below the furface of the earth, and that confequently the foil of Holland has been raised 100 feet by the tediment of the fea. '3. We may draw an induction that this strata of thick clay of 102 feet, and the bed of fand below it, in which they dug to 31 feet, and whose entire thickness is unknown, are perhaps not very far diffant from the first strata of the true antient and original earth, such as it was at its first formation, and before the motion of the water had changed its furface. We have faid in the first article, that if we defired to find the ancient earth, we should dig in the northern countries, rather than towards the equator, in low plains, rather than in mountains or high lands. These conditions are in this place found to be nearly collected together, only it is to be wished they had continued the digging to a greater depth, and that the author had informed us, whether there were not shells and other marine productions in that bed of clay, of 102 feet thick, and in that of fand below it. This example confirms what we have already faid; and the more we dig into the internal part of the earth, the more we shall find thick strata, which is very naturally explained in our theory. Kkk Not VOL. V.

Not only the earth is composed of parallel and horizontal beds, in the plains and hills, but even the mountains are in general composed after the same manner: it may be said, that these strata are more apparent there than in the plains, because the plains are generally covered with a very considerable quantity of sand and earth, which the water brought there, and in order to find the antient strata, we must dig deeper in the plains than in the mountains.

I have often observed, that when a mountain is equal, and its fummit level, the strata or beds of ftone, which compose it are also level; but if the fummit of the mountain is not placed horizontally, and if it inclines towards the east, or towards any fide, the strata of stone inclines also on the same side. I have heard many perfons fay, that in general the banks or beds of quarries inclined a little to the fide of the Levant, but having myself observed all the quarries and chains of rocks which offered, I discovered this opinion to be false, and that the strata or banks of stone only incline towards the Levant, when the top of the hill inclines to the fame fide; and that on the contrary, if the top tends to the north, fouth, &c. the beds of stone incline so likewise. When we dig stone and marble from the quarry, we take great care to feparate them according to their natural pofition, and we cannot even get them of a large fize, if we cut them in any other direction. Where they are made use of for masonry to be good, and the stone to endure a long time, we must place them on their Lit de Arriere, for so the workman call the horizontal strata: if in masonry the stones were placed in any other direction, they would split, and would not fo long refift the weight with which they are loaded. We perceive that this perfectly confirms that stones are found in parrallel and horizontal frata, which are fuccessively heaped one on the other, and that these strata composed masses where resistance is

greater in that direction than in any other.

On the whole, every strata, whether horizontal or inclined, has an equal thickness throughout its whole extent: that is to fay, every bed of any matter whatfoever, taken feparately, has an equal thickness throughout its whole extent; for example, when the bed of stone in a quarry is three feet thick in one part, it will have the fame thickness throughout : if in one part it is found to be fix foot thick, it will be fo throughout. In the quarries about Paris the bed of good stone is not thick, and scarcely 18 or 20 feet thick; in other quarries, as those of Burgundy, the stone is much thicker; it is the same with marble: the black and white marble have a thicker bed; the coloured are commonly thinner, and I know beds of very hard stone, which the farmers in Burgundy make use of to cover their houses, that are not above an inch thick. The thickness of different beds, therefore, are different, but each bed preferves the fame thickness throughout its extent; in general it may be faid, that the thickness of the horizontal strata is so greatly varied, that it is found from one line and lefs to 1, 10, 20, 30, or 100 feet thick; the ancient and modern quarries which are horizontally dug; the perpendicular and other divifions of mountains, prove that there are extenfive firata in all directions. "It is thoroughly proved, fays the historian of the academy, that all strata has formerly been a fost paste, and as there are quarries almost in every part, the surface of the earth has therefore been in all these places, muddy, at least to a certain depth. The shells found almost in all quarries prove, that this mud was moistened by the water of the sea, and confequently that the fea covered all thefe places, and it could not cover them without also covering all that was level with or lower than it; it could not

else cover every place where there were quarries, nor those which are level or lower, without covering the whole face of the terrestrial globe. We do not here consider the mountains which the sea must have also covered, since there are often found quarries and shells there: if we should suppose them formed there,

our reasoning would become stronger.

"The fea, continues he, therefore covered the whole earth, and from thence it proceeds that all the banks and beds of stone which are in the plains, are horizontal and parallel: Fish must have also been the most ancient inhabitants of the globe, which then had neither terrestrial animals nor birds. But how did the iea retire into these great vacancies, into the vaft basons which it at present occupies? What prefents itself the most natural to the mind, is, that the earth, at least at a certain depth, was not entirely folid, but intermixed with fome great vacuums, whose vaults were supported for a time, but at length funk in fuddenly: then the waters must have fallen into these vacancies, filled them, and left naked a part of the earth's furface, which became an agreeable abode to terrestrial animals and birds. The shells in quarries perfectly agree with this idea; for befides, as only the stony parts of fish could be preserved till now, we know that generally shells are heaped up in great abundance in certain parts of the fea, where they are immoveable, and form a kind of rock, and could not follow the water, which fuddenly forfook them: this is the reason that we find more shells than bones or impressions of the fish, and this even proves a fudden fall of the fea into its basons. At the fame time as our supposed vaults gave way, it is very possible that other parts of the globe were raifed, and for the fame reason there would be mountains placed on this furface, with quarries already formed; but the beds of these quarries have not preferved the horizontal direction they before had, at least least that the mountains were not raised precisely according to an axis perpendicular to the surface of the earth, which could but happen very seldom: so also as we have already observed in 1708 the beds of quarries of mountains are always inclined to the horizon, and parallel with each other; for they have not changed their position with respect to each other, but only with respect to the surface of the earth.

See the Mem. of the Acad. 1716, page 14.

These parallel strata, these beds of earth and stone. which have been formed by the sediment of the sea. often extend to very confiderable diffances, and even we often find in hills separated by a valley the same beds, and the same matters at the same level. This observation which I have made agrees perfectly with that of the height of the opposite hills, which I shall speak of presently; we may easily be affured of the truth of these circumstances, for in all narrow vallies, where rocks are discovered, we shall find the fame beds of stone and marble are found on both fides of the fame height. In a country where I frequently refide, and where I narrowly examined the rocks and quarries, I found a quarry of marble which extended more than 12 miles in length, and whose breadth was very confiderable, although I have never been able precifely to affure myself of this extent in breadth. I have often obferved, that this bed of marble is of the same thickness, and in hills divided from this quarry by a valley of 100 feet depth, and quarter of a mile in breadth, I found the fame bed of marble at the fame height; I am perfuaded it is the fame in every stone or marble quarry, where shells are found; for this observation does not hold good in quarries of grés. In the course of this work, we shall give reasons for this difference, and mention why gres is not disposed, like other matters, in horizontal beds, and and that it is in irregular blocks, both for form and

position.

We have likewise observed that the beds of earth are the same on both fides the straits of the sea, and this observation, which is important, may lead us to discover, the land and islands which have been feparated from the continent, it proves, for example, that England has been divided from France, Spain from Africa, Sicily from Italy, and it is to be wished, that the fame observation had been made in all the straits. I am perfuaded, that we should find it almost every where true, and to begin by the longeft frait known, i. e. that of Magellan, we do not know whether the same beds of stone are found of the fame weight on both fides, but we fee by the inspection of the particular maps of this strait, that the two high coasts which confine it, form nearly like the mountains of the earth correspondent angles, and that the faillant angles are opposite to the returning angles, in the turnings of the strait, which proves that the Terra del Fuega, must be regarded as part of the continent of America; it is the same with Forbishers strait, the island of Frisland, appears to have been divided from the continent of Greenland.

The Maldivian islands are only separated by small tracts of sea, on each fide of which banks and rocks are found composed of the same matter; all these islands which taken together, are near 200 miles long, formed anciently only one land; they are now divided into 13 provinces, called Atollons. Each Atollon contains a great number of small islands, most of which are fometimes overflown, and fometimes dry, but what is remarkable, these 13 Atollons are each furrounded with a chain of rocks of the fame nature as stone, and that there are only three or four dangerous inlets, by which they can be entered. They are all placed one after the other, and it evidently appears that these islands were formerly a long mountain, capped with rocks. (See the voyages of Francis Piriard, Vol. 1. Paris 1719, page 108,

&c.)

Many authors, as Verstegan, Twine, Somner, and especially Campbell, in his description of England, in his chapter of Kent, gives very strong reasons, to prove that England was formerly joined to France, and been separated from it by an effort of the sea, which opened that channel, left a great quantity of low and marshy ground naked, all along the southern coasts of England. Dr. Wallis, as a proof of this circumstance, shews the conformity of the antient Gallic and British tongues, and adds many observations which we shall relate in the sol-

lowing articles.

If we confider the form of ground, the pofition of mountains and the finuofities of rivers, we shall perceive that generally opposite hills are not only composed of the same matters, on the fame level, but are nearly of an equal height. This equality I have observed in my travels, and have nearly found the same, on the two sides, especially in valleys, which are no more than a quarter or a third of a mile broad; for in great valleys which are very broad, it is very difficult to judge of the height and equality of hills, because there is both a deception in optics and judgment. By looking on a plain or any other level ground which extends any distance, it appears to rise; and on the contrary, by looking on hills at a distance they appear lower; but this is not the place to give a mathematical reafon for this difference. On the other hand, it is very difficult to judge by the naked fight, where the middle of a great valley is, at least if there is no river; whereas in confined vallies our fight is less equivocal and our judgment more certain. That part of Burgundy comprehended by Auxerre, Dijon, Autun

Autun and Bar-fur-seine, a considerable extent of which is called the bailliage de la Montagne, is one of the highest parts of France; on one fide of most of these mountains which are only of the second class, and should be regarded only as high hills; the water flows towards the Ocean, and on the other fide towards the Mediterranean. There are points of division as at Sombernan, Pouilli in Auxois, &c., where the water may be indifferently turned towards the Ocean or Mediterranean. This high country is cut with many fmall vallies, very confined, and almost all watered with large rivulets or small rivers. I have a thousand and a thousand times observed the correspondence of the angles of these hills and their equality of height, and I am certain, that I have every where found the faillant angles opposite to the returning angles, and the lengths nearly equal on both fides. The farther we advance into the higher country, where the points of division are; the higher the mountains are; but this height is always the same on both sides of the vallies, and the hills are raised or lowered alike. By placing myself at the extremity of avalley towards the middle of its breadth, I have always observed that the hollow of the valley was furrounded and furmounted with hills of an equal height; I made the like observation in many other parts of France. It is this equality of height in the hills which form plains in mountains; there plains form, as we may fay, lands higher than others. But high mountains do not appear so equal in height, most of them terminate in points and irregular peaks, and I have feen in crofting the Alps and the Apennine mountains, that the angles, are in fact correspondent, but it is almost impossible to judge by the eye of the equality or inequality of opposite mountains, because their summits are lost in mist and clouds. sulfer whereas in cooking basis aways

The different strata of which the earth is compofed, are not disposed according to the order of their spespecific weight; for we often find strata of heavy matters placed on strata of lighter. To be affured of this, we have only to examine the nature of the. earth on which rocks are placed, and we shall find that it is generally clay which is specifically lighter than the matter of the rock. In hills and other fmall elevations, we eafily discover the base on which rocks are placed; but it is not fo with large mountains, not only the fummit is rock, but those rocks are placed on other rocks; there are mountains upon mountains and rocks upon rocks, to fuch a confiderable height, and in fo great an extent of ground, that we can fearcely be certain where there is earth at bottom, and of what nature it is. We see picked rocks which are many hundred feet high; these rocks rest on others, which perhaps are no less; nevertheless, may we not compare great wh small? and fince the rocks of little mountains, whose bases are to be feen, rest on earth less heavy and folid than stone, may we not suppose that the base of high mountains is also of earth? On the whole, all that I have here to prove is, that by the motion of the waters, it may naturally happen that the more ponderous matters accumulated on the lighter; and, that if this in fact is found to be fo in most hills, it is probable that it has happened as I explain it; but if my reasons should be rejected, by objecting that I am not properly grounded to suppose, that before the formation of mountains the heaviest matters were below the lighter; I answer, that I affert nothing general in this respect, because this effect may have been produced in many manners; whether the heaviest matters were uppermost or undermost, or placed indifferently, we at present see them. For to conceive how the sea at first formed a mountain of clay and afterwards capt it with rocks; it is sufficient to confider the sediment may successively come from different parts, and that they may be of different VOL. V. matmatters, so that in some parts of the sea where the water shall have deposited at first many sediments of clay, it may very likely happen, that instead of clay the waters brought a stony sediment, and that because they have raised the clay from the bottom and loosened it from the sides, so that afterwards they attached themselves to the rock, or possibly because the first sediment came from one part, and the second from another. On the whole, this perfectly agrees with observation, by which we perceive that the beds of earth, stone, gravel, sand, &c. sollows no rule in their arrangement, or at least are placed indifferently one on the other as it were by chance.

Nevertheless, even this chance must have rules, which can be known only by estimating the value of probabilities, and the truth of conjectures. By attending to our hypothesis, on the formation of the globe, we have feen that the internal part of the earth must have been a vitrified matter, similar to our vitrified fands, which are only fragments of glass, and of which the clays are perhaps the scoria; in this supposition, the center of the earth, and almost as far as the external circumference, must be glass, or a vitrified matter, which fills almost all the internal part, and above this matter we should find fand, clay, and other scoria of this vitrified matter; thus by confidering the earth in its first state, it was a nucleus of glass or vitrified matter, which is either massive like glass, or divided like sand, because that depends on the degree of vehemence of the fire it has undergone: above this matter were fand, and at last clay; the soil of the waters and air produced the external crust, which is thicker or thinner according to the fituation of the ground, more or less covered according to the different mixtures of foil, fand, and animal and vegetable parts; and more or less fertile according to the abundance or want of these parts. To shew that this supposition on the formation of sand and clay, is not so chimerical as may be imagined, we have thought

proper to add fome particular remarks.

I conceive therefore, that the earth in its first state was a globe, or rather a spheroid of vitrisied matter, and very compact glass, covered with a light and friable crust, formed by the scoria of the matter in suspense such a pumice stone. The motion and agitation of the waters and air soon broke and reduced this crust of spungy glass to powder. Hence the sand, which by uniting afterwards produced large slints, which as well as the small, owe their hardness, colour or transparency and variety to the different degrees of purity, and sineness of the grain of the sand which entered into their composition.

These sands whose constituting parts united by fire. affimilated, and became a very dense hard body, so much the more transparent as the fand was more homogenous; on the contrary, being exposed a long time to the air, they decomposed by the disunion, and exfoliation of the small lama of which they were formed, they begin to become earth, and it is thus that clay and potter's earth are formed. This dust, sometimes of a brightish yellow, and sometimes like filver fand, is nothing else but a very pure fand somewhat perished, almost reduced into its principles, and which tends to a perfect decomposition. By time, this fand will be fo far attenuated and divided, that they will no longer have sufficient thickness, and surface to reflect the light, and acquire all the properties of clay: this theory is confirmable to what every day is feen; let us immediately wash fand as foon as dug, and the water will be loaded with a black ductile In cities where the streets are and fat earth. paven with gre's stone, the dirt is always black and greafy, and dried forms an earth of the fame nature as potter's earth. Let us moisten and wash th's earth likewise taken from a spot where there are neither gre's gre's nor flints, there will precipitate a great quan-

tity of vitrifiable fand.

But what perfectly proves that fand and even flint and glass exist in this earth, and are not disguised therein, is, that the fire, by uniting the parts of the latter, which the action of the air and other climates had possibly divided, restores it to its first form. Argile put in a reverberating furnace, heated to the degree of calcination, it will cover itself externally with a very hard enamel; if it is not vitristed internally, it nevertheless will have acquired a very great hardness; it will resist the file, it will omit fire under the hammer, and it has all the properties of slint, a greater degree of heat causes it to slow, and converts

it into real glass.

Argileand fand are therefore matters perfectly analogous, and of the fame class: if argile by condensing may become flintand glass, why may not fand by dividing become potter's earth? glass appears to be true elementary earth, and all the mixt a difguifed glass: metals, minerals, falts, &c. are only vitrescible earth; common stone and other matters analogous to it, and testaceous, and crustaceous shells, &c. are the only fubstances which not be vitrified by any known agent and the only ones which feem to form a feparate class. Fire by uniting the divided parts of the first, forms an homogenous matter, hard, and transparent to a certain degree, without any diminution of weight, and to which it is no longer possible to cause any alteration; those on the contrary, in which a greater quantity of active and volatile principles enter, and which calcine, lose more than one-third of their weight in the fire, and fimply retake the form of earth, without any alteration than a difunion of their principles: these matters excepted, which are no great number, and whose combinations produce no great varieties in nature; every other substance, and particularly potter's earth, may be converted into glass, and are confequently effentially, only a decomposed glass. If the fire fire suddenly causes the form of these substances to change, by vitrifying them, glass itself, whether it has its glassy nature, or whether that of sand or flint, naturally changes into potters earth, but by

a flow and intentible progrefs.

In the foil where flint is generally the predominant stone; the country is generally fertile and if the place is uncultivated, and thefe stones have been a long time exposed to the air, without having been flirred; their upper superficies is always very white, whereas the opposite fide, which touches the earth, is very brown, and preferves its natural colour; if many of these flints are broken, we shall perceive that the whiteness is not only external, but penetrates internally, and there forms a kind of band, not very thick in certain flints, but which in others occupies almost the whole flint. This white part is fomewhat grainy, entirely opaque, as foft as stone, and adheres to the tongue like bole; whereas other flint is fmooth, has neither thread nor grain and preferves its natural colour, transparency and hardness; if flint is put into a furnace, its white part becomes of a brick colour, and its brown part of a very fine white. Let us not fay with one of our most celebrated naturalists, that these stones are imperfect flints of different ages, which have not acquired their perfection; for why should they be all imperfect? why should they be all on the same side, and exposed to the weather? It appears to me very reasonable that there are on the contrary slints, changed and decomposed, and which assumes the form and property of bole and potters earth, of which they were formed. If this is thought to be only conjecture, let the hardest, blackest, and most flinty flint, (as this famous naturalist terms it) be exposed to the weather, in less than a year, its surface will change colour, and if we have patience to purfue this experiment, we shall see it by degrees lose its hardness, transparency and other specific characters, and approach every day nearer, and nearer the nature of argile.

What happens to flint, happens to fand; each grain of fand may be confidered as a small flint, and each flint as a mass of grains of sand, extremely fine, and exactly grained. The example of the first degree of decomposition of fand is found in the brilliant and opaque powder called Mica, in which potters earth and flate are always diffused. The entirely transparent flints, the Quartz, produce, by decomposition, fat and foft talk, as petrifiable and duffable as clay: and it appears to me that talk is a mediate term between glass or transparent flint and argile; whereas coarfe and impure flint, by decomposing, passes to

potters earth without any intermedium.

Our factitious glass proves also the same alterations: it decomposes in the air, and perishes in some manner by remaining in the earth. At first its fuperficies fcales exfoliates, and by working it we perceive brilliant fcales fly from it; but when its decomposition is more advanced, it crumbles between the fingers, and is reduced into a very white fine talky powder. Art has even imitated nature in the decomposition of glass and flint. Est etiam certa methodus solius aquæ communis ope silices & arenam in liquorem vifcosum, eumdemque in sal viride convertendi, & hoc in oleum rubicundum, &c. Solis ignis & aquæ ope speciali experimento durissimos quosque lapides in mucorem resolvo, qui distillatus subtilem spiritum exhibet & oleum laudibus prædicabile. See Becher, Phys. subter.

Thefe matters more particularly belong to minerals, and we shall content ourselves here with adding, that the different strata which cover the terreftrial globe, being actually either matters to be confidered as vitrified, and which are all vitrifcible, and as it is evident that from the decomposition of glass and flint, which is every day made before our eyes, a true argilous earth remains; it is not therefore a precarious and hazardous supposition to advance

that clays, argile, and fands have been formed by the fcoria, and vitrified drops of the terrestrial globe, especially when we join the proofs a priori, which we have given to evince there has been a strata of liquesaction caused by fire.

PROOFS of the THEORY of the EARTH.

ARTICLE VIII.

On Shells, and other Marine Productions, found in the Bowels of the Earth.

Have often minutely examined quarries, the banks of which were filled with fhells; I have feen entire hills composed of them, and chains of rocks which contained them abundantly throughout their whole extent. The volume of these marine productions is aftonishing, and the number fo prodigious, that it is fcarcely possible, that there can be more in the fea; it is by confidering this innumerable multitude of shells, and other marine. productions, that leaves us no doubt of our earth's having been a very long time under water, and inhabited by as many shell fish as it at present is. The quantity is immense, and naturally it might not be imagined that there was fo great a multitude of these animals in the sea; it is not by the fosfil and petrified shells found on the earth, that we can have an idea of them. In fact, we must not think, as those people imagine, who reason on these matters, without having seen them, that these shells are only found at random, dispersed here and there, or at most in small heaps, as oyster shells thrown before our doors; it is in mountains they are met with, in shoals of 100 or 200 miles length; it is in hills and provinces we must look for them, often 50 or 60 feet thick, and it is from these circumstances we must reason.

We cannot give a more striking example on this subject than the shells of Touraine. The following is what the Historian of the Academy fays, anno 120, page 5, &c. "In all the ages fo unenlightened and deprived of the genius and observation and refearches, to think, that all what is called figured stones, and the shell fish found in the earth, were the sports of nature, or some trivial particular accidents, chance has brought to light an infinity of these curiofities, which even philosophers, (if there were philosophers) regarded but with an ignorant furprize, and a flight attention, and all this perished without any fruit for the progress of knowledge. A potter who knew neither Latin nor Greek, towards the end of the 16th century, was the first who dared affirm in Paris, to the face of all the doctors, that the fossil shells were real shells formerly deposited by the sea where they were then found; that animals, and particularly fish, had given to stones all these different figures, &c. and he boldly defies the whole school of Aristotle to attack what he fays. This was Bernard Paliffy Saintongeois, as great a physician as nature could form; nevertheless his system slept near 100 years, and even the author's name is almost dead. length the ideas of Paliffy are revived in the mind of feveral fages; they have made the fortune they deferved, they have profited by all the shells and figured stones the earth furnishes us with; perhaps they are at prefent become only too common, and the consequences drawn from them are soon in danger of being very incontestable.

Notwithstanding this, the observations presented by M. Reaumur must appear wonderful; a mass of 130 millions 680 thousand cubical fathom, dug under the earth, and which was one mass of shells or fragments of shells, without any mixture of shone, earth, sand, or other extraneous matter:

hitherto

hitherto fossil shells have never appeared in such an enormous quantity, nor have they ever appeared in a much less quantity without mixture. It is in Tourain this prodigious mass is found more than 36 miles from the sea; this is perfectly known there, as the farmers of this province make use of these shells which they dig up, as marl for their lands, to fertilize their plains which otherwise would be absolutely sterile. We leave to M. Reaumur to explain this particular, and to all appearance absurd mode succeeded, we shall inclose ourselves in the singularity of this great heap of shells.

"What is dug from the earth, and which generally is no more than 8 or 9 feet deep, is only small fragments of shells, very distinguishable as fragments, for they have very perfect hollows, having only lost their gloss and colour, as almost all shells do which we find in the earth. The smallest pieces, which are only dust, are still distinguishable, because they are perfectly of the same matter as the rest, and sometimes whole shells are found. We discover the kinds as well in the whole shells as in the larger fragments; some of these kinds are known at Poitou, others belong to more remote coasts. There are even fragments of marine stony plants, such as Madrepores, sea mushrooms, &c. all this matter in the country is termed Faltun.

"In physical points, the smallest circumstances, which most people do not think worthy of remarking, sometimes lead to consequences, and afford great lights. M. de Raumur observes, that all the fragments of shells are in their heaps placed stat, and horizontally, and hence he has concluded that this infinity of fragments does not proceed from the heap being formed of whole shells, the uppermost by their weight have crushed the others, for according to that fallings would have ensued which would Vol. V. M m m

have given an infinity of different positions. All these shells must have been brought there by the sea, either whole or broken, and as they were brought floating, they were placed flat and horizontally; after they were thus all deposited in the common rendezvous, the extreme length of time broke some, and almost calcined the greatest part without deranging their position?

"By this it appears, that they must have been brought successively, and in fact how could the sea carry at once such a prodigious quantity of shells, all in an horizontal position? they must have collected in one spot, and consequently this spot must have been the bottom of a gulph or kind of bason.

remained, and effectually does remain on the earth, many vestiges of the universal deluge related in the holy scripture, the mass of shells at Touraine was not produced by this deluge; there is perhaps not so great a mass in any part of the sea: but in short, the deluge did not tear them away, and if it had, it would have been with an impetuosity and violence which would not have permitted all these shells to have one uniform position. They must have been brought and deposited gently, and slowly, and confequently in a space of time much longer than a year.

Therefore, the furface of the earth must have been, before or after the deluge, very differently disposed from what it at present is: that the sea and continent had another arrangement, and that in short there was a great gulph in the middle of Touraine; the changes which are known to us and which have something historical, since the sabulous ages, are in sact but inconsiderable, but they give us room easily to imagine those which a longer time might bring about. M. Reaumur has a supposition how the gulph of Tourain communicated with the

ocean,

ocean, and what the current was that conveyed the shells there; but this is only a simple conjecture laid down in room of the real unknown sact. To speak certainly on this matter, we should have geographical maps made according to all the species of shell sish dug in the earth. What a quantity of observations would it not require, and what a time to make them? Who knows nevertheless whether the sciences will not in suture reach as far, or at

least patt of the way towards it.

"This very confiderable quantity of shells will astonish us less, if we consider some circumstances, which is proper not to omit; the first is, that shell fish multiply prodigiously, and increase in a very fhort time; the attendance of individuals in each kind proves to us their fecundity. We have a frong example of this multiplication in oysters; sometimes in a fingle day a mass of many fathoms of these shell fish are raised. In a very short time, the rocks from which they are separated are considerably diminished, and other places where they are fished for exhausted likewise; nevertheless the enfuing year we find them as plentiful as before, the quantity of oysters are not perceived to be diminished, and I know not whether the parts where they naturally come, were ever entirely exhausted. fecond confideration is, that the substance of the shells is analogous to stone, that they are a long time preferved in foft matters, and petrify readily in hard, that these marine productions and shells found on the earth, being the wrecks of many ages, must have formed a very considerable volume.

There are, as we perceive, a prodigious quantity of shells in marble, lime, stone, chalk, marl, &c. we find them as I before said, in hills and mountains, and often they make more than one half of the volume of matters which contain them; for the most part they appear well preserved, others

are in fragments, but large enough to distinguish by the eye to what kind of shell these fragments belong, and here the observations and knowledge inspection may give us is limited. But I shall go further, and affert that shells are the intermedium which nature uses to form stone. I affert, that chalks, marls, and lime stone are composed only of the powder and detriments of shells; that confequently the quantity of shells destroyed are infinitely more confiderable than that of preserved shells; I shall here content myself with indicating the point of view in which we must consider the strata of which the globe is composed. The first external strata is composed of the dirt of the air, the sediment of the rain, dew, and vegetable or animal parts, reduced to particles, in which the antient organisation is not perceptible; the internal strata of chalk, marl, lime, stone, and marble, are composed of the ruins of shells, and other marine productions, mixed with fragments or whole shells, but the vitrifiable fand or argile are the matters of which the internal part of the globe is composed, They were vitrified when the globe received its form, which necessarily supposes that the matter was in fusion. The granite, rock, flint, &c. must owe their origin to fand and argilous earth, and they are likewise disposed by strata, but tuffs *, gres, and flints, which are not in great maffes, chrystals, metals, pyrites, most minerals, sulphurs, &c. are matters whose formation is novel, in comparison with marbles, calcinable stones, chalk, marl, and all other matters disposed in horizontal strata, and which contain shells and other ruinated productions of the fea.

As the denominations I make use of might appear obscure or equivocal, I judged it necessary to explain them. I understand by the word argile not only the white yellow, but also the blue, soft, hard, foliated

^{*} A fort of fost gravelly stone.

foliated and other clays, which I look on as the fcoria of glass, or as decomposed glass. By the word fand, I always understand vitrifiable fand, and not only comprehend under this denomination the fine fand which the gres produce, and which I look upon as powdered glass, or rather pumice stone; but also the sand which proceeds from the gres destroyed by friction, and also the larger fand as small gravel which proceeds from the granate, which is tharp angular, red, and very commonly found in the bed of rivers or rivulets, which drive their waters immediately from the higher mountains, or hills of stone or granate, The river Armanson conveys a great quantity of this fand, which is large and very tharp, and in fact, is only fragments or calcinable gravel, or the fragments of stone. On the whole, rock stone and granate are one and the same fubstance; but I thought it my duty to make use of both denominations, because there are many perfons who make two different matters of them. It is the fame with respect to flints and gres, in large lumps; I look on them as kinds of granate, and I call them large flints; because they are disposed like calcinable stone in strata, and to distinguish them from the flints and gres, which I term in small masses, and which are round flints and gres, which are found a la chasse, as the workmen say; that is to say, the gres where banks have no regular courfe, and do not form continued quarries, and which have a certain extent. These gres and flints are of a more modern formation, and have not the fame origin as the flints and gres in large lumps, which are disposed in strata. I understand by flate, not only the blue slate which all the world knows, but white, grey, and red flate: these matters are generally met with above the foliated argile, the different small strata of which received a body in drying, which has produced the beds,

beds we find there. Pit coal, jet, &c. are matters which also belong to argile. By the word tuf, I understand not only the common tuf which appears, and as I may fay organised, but also all the strata of stone made by the deposit of running waters, all the stalactites, incrustations, and all kinds of fufing stone. It is no ways dubious that these matters are not modern, and that they every day grow. The tuf is only a mass of lapidific matter, in which we perceive no diffinct strata; this matter is disposed generally in small hollow cylinders, irregularly grouped and formed by waters dropt at the foot of mountains, or on the flope of hills, which contain beds of marl or foft and calcinable ftone; the total mass of these cylinders, which make one of the specific characters of this kind of tuf, is either oblique or vertical, according to the direction of the streams of water which form them, Thefe fort of parafite quarries have no continuation; their extent is very confined in comparison of the common quarries, and proportionate to the height of the mountains which furnish them with the matter of their growth. The tuf every day receiving lapidific juices, those small cylindrical columns between which intervals are left, close at last, and the whole becomes one compact body; but this matter often acquires the hardness of stone, and is what Agricola terms Marga tofacea fiftulofa. In this tuf is generally found a quantity of impressions of leaves of trees and plants, like those which grow in the environs : terrestrial shells also are often met with, but never any of the marine kind. The tuf is certainly therefore a new matter, which must be ranked in the class of stalactites, incrustations, &c. all these new matters are kinds of parafite stones, formed at the expence of the rest, but which never arrive at true petrification.

Chrystal,

Chrystal, precious stones, and all those which have a regular figure, even small slints formed by concentrical beds, whether found in perpendicular cavities of rocks, or elsewhere, are only exudations of large slints, concrete juices of the like matters, new parasite stones, and real stalactites of slint or rock.

Shells are never found either in rock, granate, or gres, at least I have never seen them there, although they are very often met with in vitrisiable sand, from which these matters derive their origin, which seems to prove that sand cannot unite to form gres or rock but when it is pure, and that if it is mixed with substances of another kind, as shells are, this mixture of parts which are heterogeneus to it, prevents the union. I have observed these little pebbles which are often found in beds of sand, mixed with shells, but never found any shell therein: these pebbles are real gres, concretions formed in the sand in the places where it is not mixed with heterogenous matters, which oppose the formation of bands or other masses larger than these.

We have before faid, that at Amsterdam, which is a very low country, fea shells have been found at 100 feet depth below the earth, and at Marly-laville, 6 miles from Paris, at 75 feet, we likewife meet with the fame at the bottom of mines, and in banks of rocks, beneath a height of stone 50, 100, 200, and 1000 feet thick, as is eafy to be remarked in the Alps and Pyrenees. We have only narrowly to inspect, and we shall find in the lower beds shells and other marine productions. But to proceed in order, we find on the mountains of Spain, the Pyrenees, the mountains of France, or those of England, in all the marble quarries of Flanders, in the mountains of Guelders, in all hills around Paris, Burgundy, and Champagne; in one word, in every part where the depth of foil is not gres or tuf; and in most of these places there is almost more shells. other matters in the stones. By shells L not only understand the wrecks of shell sish, but those of crustaceous, and also all productions of sea infects, as coral, madrepores, astroites, &c. I can affert, and we may be convinced by inspection, that in most calcinable stone and marble, there is so great a quantity of these marine productions that they appear in volume to surpass the matter which unites them.

But let us proceed; we met with these marine productions, even above the highest mountains; for example, on Mount Cenis, in the mountains of Genes, in the Apennine, and in most of the stone and marble quarries in Italy. In the stones of the most antient edifices of the Romans. In the mountains of Tirol, and in the center of Italy, on the summits of mount Paternus, near Bologne; and in those parts where that luminous stone, called Bolognian stone is produced, in the hills of Calabria, in many parts of Germany, and Hungary, and generally in all the high parts of Europe. On this subject see Stenon, Ray, Woodward, and others.

In Afia and Africa, travellers have remarked them in feveral parts; for example, on the mountains of Castravan above Barut, there is a bed of white stone as thin as slate, each leaf of which contains a great number and diversity of sishes; they are for the most part very slat, and compressed, as is the sossil fern, but they are notwithstanding so well preserved, that the smallest traces of the sins, scales, and all the parts which distinguish each kind of sish,

are perfectly visible.

So likewise we find many sea muscles, and petrified shells between Suez and Cairo, and on all the hills and eminences of Barbary. The greatest part are conformable to the kinds actually caught in the Red sea, (See Shaw's voyages, Vol. 11, page 70, and

14) In Europe we meet with petrified fish in Sweden and Germany, in the quarry of Oningen, &c.

The long chain of mountains, fays Bourquet, which extends from east to west, from the extremity of Portugal to the most eastern parts of China, those which extend colaterally from north to south, the mountains of Africa and America, the vallies and plains of Europe, all inclose strata of earth, and stones filled with shell fish, and from hence we may conclude the same of all the other parts of the world unknown to us.

The islands of Europe, Asia, and America, where the Europeans have had an opportunity to dig, whether in mountains or plains, furnish shells which evince that they have that in common with the bordering continents.

Here then is fufficient to prove, that fea shells, petrified fish, and other marine productions, are to be found in almost every place we are disposed to feek them.

"It is certain (fays an English author, (Tancred Robinson) that there has been sea shells dispersed here and there on the earth by armies, inhabitants of towns and villages, and that Loubere relates in his travels to Siam, that the monkies of the Cape of Good Hope continually amuse themselves with carrying shells from the sea shore to the tops of the mountains, but that cannot resolve the question, why these shells are dispersed over all the earth, and even in the interior parts of mountains, where they are deposited in beds at the bottom of the sea."

By reading an Italian letter on the changes happened to the terrestrial globe, printed at Paris this year 1746, I expected to find this circumstance related there by Loubere; it perfectly agrees with this author's ideas. Petrified fish, according to his opinion, are only fish, rejected by the Roman table because they were not fresh, and with respect

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to shells, he fays the pilgrims of Syria brought, during the times of the Crusades, those of the Levant Sea, into France, Italy, and other Christian states: why has he not added that it was the monkies who transported the shells to the tops of the mountains, and in every place where men cannot dwell; this would not have spoiled, but rendered his explanation still more probable. How comes it that enlightened perfons, who pique themselves even on philofophy, have nevertheless such absurd ideas on this fubject? We shall not therefore content ourselves with having faid, that petrified shells are found in almost every part of the earth, which has been dug; nor with having related the testimonies of authors of natural history; for as it might be suspected, that with a view of fome fystem, they perceived shells where there was none, we think it our duty fill to quote travellers, who have remarked them accidentally, and whose fight, not so well accustomed to these matters, have only discovered whole and well-preferved shells; their testimony will perhaps be of a still greater authority with people who have it not in their power to be affured of the truth of these facts, and of those who know neither shells nor petrifications, and who not being in a flate to compare them, might suspect that these petrifications were real shells, and whether these shells are found heaped up in millions throughout the whole earth.

All the world may see the banks of shells in the hills, in the environs of Paris, especially in the quarries of stone, like those at Chaussee, nigh Seve at Issy, Passy and elsewhere. We find a great quantity of lenticular stones at Villers-Cotterels; these rocks are entirely formed thereof, and they are blended without any order with a kind of stony mortar, which binds them together. At Chaumont so great a quantity of petrified shells are found, that the hills appear to be composed of nothing else. It is the

fame

fame at Courtagnon, near Rheims, where the banks of shells is near four miles broad by many long. I quoted these places as being famous, and striking the eyes of every one.

With respect to foreign countries, here follows

the observations of some travellers.

"In Syria and Phenicia, the stone which serves as a basis to the rocks in the neighbourhood of Latikee, is surmounted with a kind of soft chalk, and it is perhaps from thence that the city has taken the name of the white promontory. Nakoura, anciently termed Scala Tyriorum, or the Tyrians Ladder, is nearly of the same nature, and we still find there, by digging, quantities of all forts of shells, corals, &c.

(See Shaw's travels.)

On mount Sina we find only a few fossil and other shells the marks of the deluge, at least if we do not rank the fosfil Tamarin of the neighbouring mountains of Siam among this number, perhaps the first matter of which their marble is formed, had a corrofive virtue not proper to preserve them. But at Corondel, where the rock approaches nearer our stone, I find many shells, as also a very fingular fea muscle, of the kind called Spatagi, but closer and rounder. The ruins of the little village Ain el Mousa, and many canals which conduct the water thereto, furnish fosfil shell fish. The ancient walls of Suez and what yet remains of its ancient part have been constructed of the same materials which feem to have been taken from a like part. Between, as well as on all the mountains, eminences and hills of Lybia which are not covered with fand, we meet with a great quantity of fea weed, as well as vivalvous shells, and of those which terminate in a point, most of which are exactly conformable to the kinds at prefent caught in the red fea.

The moving fand in the neighbourhood of Ras Sem, in the kingdom of Barca, cover many palm

trees with petrifications. Ras Sem signisses the head of a sish, and is what we term the petrified Village, where it is said, men, women and children are found in several postures and attitudes, who with their cattle, aliments and goods, have been converted into stone? but excepting these monuments of the deluge, which are here spoken of, and which are not particular to this part, all the rest of it are vain tales and sables, all which I have not only learnt from M. le Maire, who at the time he was consul at Tripoly, sent several persons thither to take cognizance of it; but also from very respectable persons who had been at those places.

Before the pyramids certain pieces of stone, worked by the sculptor are to be found, and among these stones many rude ones of the sigure and size of lentils; some even resemble barley; now it is said, they are the remains of what the workmen ate, which does not appear probable, &c. These lentils and barley are petrisications of shells called by naturalists

the centicular ft ne.

Several forts of these shell fish which we have spoken of, are found in the environs of Maestricht, especially towards the village of Zicken or Tichen, and at the little mountain called Hans. In the environs of Sienna, I have found near Ceraldo, (according to the advice you gave me) many mountains of sand all crammed with divers forts of shells. Montemario, a mile from Rome, is entirely filled with them; I have seen them in France and elsewhere. Olearius, Steno, Camden, Speed and a number of other authors, as well ancient as modern, relate the same phenomena.

"The island of Cerigo was anciently called Porphyris, from the quantity of porphyry which was ta-

ken out of it. (Thevenot, vol. i. p. 25.)

"Opposite the village of Inchena, and on the castern shore of the Nile, I found petrified plants, which

which grew naturally in a space about two miles long, by a very moderate breadth; this is one of the most singular productions of nature. These plants resemble the white coral found in the red sea."

(Voyage of Paul Lucus, vol. 11, page 380.)

"We meet with petrifications of diverse kinds, on Mount Libanus, and among others flat stones, where the skeletons of fish are found well preserved and entire, and also red sea chesnuts, with small grains of

coral of the fame fea."

"On Mount Carmel we find a great quantity of stone, which, as is afferted, have the figure of melons, peaches, and other fruit, which are commonly fold to pilgrims, not only as mere curiofities, but also as remedies against many disorders. The olives which are the lapides judaici, are to be met with at the druggists, and have always been looked upon as a specific for the stone and gravel."

"M. la Roche, M. D. gave me some of these petrissed olives, which grew in great plenty in these mountains, where I am told are sound other stones, the inside of which perfectly resemble the natural parts of men and women. These are Hysterolithes.

"In going from Smirna to Tauris, when we were at Tocat, fays Tavenier, the heat was so great, as obliged us to quit the common road, to gain the mountains where there is constantly shade and refreshing air. In many places we found snow and a quantity of very fine forrel, and on the top of some of those mountains we found shells like as on the sea shores, which was very extraordinary."

Here follows what Olearius fays on the subject of petrified shells, which he remarked in Persia, and in the rocks where the sepulchres are cut out near to

the village of Pyrmaratus.

"We were three in company that ascended to the top of the rock by frightful precipices assisting each other; we found three or four large chambers, and within, many niches cut in the rocks to serve for

beds.

beds. But what the most surprized us, was to find in this vault on the top of the mountain, muscle shells, in some places in such great quantities, that the whole rock appeared to be composed only of sand and shells. Returning to Persia, we perceived many of these mountains and shells along the Caspian sea."

To what has been related I could subjoin many other quotations which I suppress, not to tire those who have no need of superabundant proofs, and who are convinced by their fight, as well as me, of the existence of these shells wherever we chuse to seek for them.

In France we not only find the shells of the French coaft, but also shells which have never been feen in those seas. There are even naturalists who affert, that the quantity of these foreign petrified shells is much greater than those of our climate; but I think this opinion misfounded, for independent of the shell 6th which inhabit the bottom of the fea, and of those which are difficult to fish for and which consequently may be looked on as unknown or even foreign, although they might be bred in our feas; I fee in the whole, that by comparing the petrifications with the living analogous animals, there is more of those of our coasts than of others: for example, most of the cockle kind, muscles, oysters, astroits, corals, madrepores, &c. found petrified in fo many places, are certainly the productions of our feas, and although there is found a great number of lenticular and Judaic stones, the columnities, the vertebra's of the great starfish and several other petrifications, whose living analogies are foreign or unknown; Iam convinced from my observations, that the number of these kinds is small in comparison of that of the petrified shells of our coasts: besides, what composes the bottom of our marble and almost all our stone; but Madripores, Astroites and all those other productions

ons which are formed by fea infects, and formerly called marine plants; shells however abundant, form only a small volume in comparison of these productions, which are all originated from our seas, and

particularly in the Mediterranean.

Of all the seas the red sea produces corals, madrepores and marine plants in the greatest abundance: there is perhaps, no part that furnishes a greater variety than the port of Tor. In calm weather, so great quantity of these plants present themselves, that the bottom of the sea resembles a forest. There are branched madrepores eight or ten seet high, we find many in the mediterranean sea, at Marseilles, near the coasts of Italy and Sicily. There are also in great quantity in most of the gulphs of the ocean, around islands, on banks, and in all temperate climates where the sea is but of a moderate

depth.

Pevstonel was the first who observed and discovered that corals, madrepores, &c. owed their origin to animals, and were not plants as supposed, and as their form and growth feem to indicate the observation of M. Peyffonel, was in fact a long time doubted of: fome naturalists too prejudiced with their own opinions, at first even rejected it with a kind of disdain; nevertheless they have been obliged fince, to acknowledge the discovery of M. Peyssonel, and the whole world is at length of opinion, that these supposed marine plants, are nothing else than hives or cells for small animals which resemble shell fish in their forming like them, a great quantity of slony matter, in which they dwell, as fish do in their shells. Thus marine plants, which at first were placed in the class of minerals, have fince passed into that of vegetables, and at last remained fixed in that of animals.

There are shell fish which live at the bottom of the sea, and never cast on the shore: authors call them them Pelogia, to diffinguish them from others which they call Litterates. It is to be supposed the cornus animon, and some other kinds that are sound petrissed, and to which living analogies have not been found, always remain at the bottom, and that they are filled with the stony sediment in the place they are in. There might also have been certain animals, whose species are perished, of which number this shell sish might be ranked. The extraordinary sossil bones found in Siberia, Canada, Ireland, and many other places, seems to consirm this conjecture, for no animal has hitherto been known that we can attribute such bones to, which are for the most part of an unmeasurable size.

These shells are met with from the top to the bottom of quarries; we see them also in much deeper pits: there are some at the bottom of the pits of Hungary; they are sound a thousand seet deep in the rocks, which border the isle of Calda, and in

Pembrokeshire in England.

Petrified shells are not only formed at great depths, and at the bottom of the highest mountains, but there are fome met with whose nature is not changed; and which have the glofs, colours, and lightness of sea shells; glossopetres, and other teeth of fish are found in their jaws, and to convince ourfelves of this matter entirely, we have only to look on the fea and land shell, and compare them. There is no person who after even a slight examination, can doubt that these fossil and petrified shells, are not the fame as those of the sea; the smallest articulations are remarked, and even the pearls that the living animals produced: the teeth of the fifth are remarked to be more fmooth, and used at the extremity, and that they have been made use of when the animal was alive.

We also almost every where on land meet with shell fish of the same kind, some of which are small, others others large, fome young, others old; fome imperfect, others extremely perfect; and we likewise see

fmall and young fish adhering to the large.

The shell fish called Purpura has a very long tongue, the extremity of which is bony and sharp, it serves it to pierce the shells of other fish, and to feed on the slesh; we commonly find shells in the earth pierced in this manner, which is an incontestable proof that they formerly enclosed living sish, and that these sish dwelt in those parts where there was the Purpura, which sed on them.

The obelisks of Saint Peter at Rome, Saint John, de Latran, of the square Navon e, came, as is faid from the pyramids of Egypt; they are of red granite, which is a kind of rock stone of very hard gres; this matter, as I have observed, contains no thells but the African and Egyptian marble, and the porphyry that has been cut from the temple of Solomon, and the palaces of the kings of Egypt; and that has been used at Rome in different parts, are filled with shells: the red porphyry is composed of an infinite number of fpots, which we call the ourfin, or fea chefnut: they are placed pretty near each other, and form all the small white spots which are in this porphyry. Each of these white spots has a small black one in its center, which is the fection of the longitudinal passage of the point of the ourfin. At a place called Fucin, three miles from Dijon, in Burgundy, is a red stone, perfectly similar in its composition to porphyry, and which differs from it only in hardness; very beautiful pieces of workmanfhip have been made of it in this province, and particularly the steps of the pedestal of the equestrian statue of Louis le Grand, which is in the royal square at Dijon. This stone is not the only one of the kind which I know. In the province of Burgundy, near Belfast, there is a considerable quarry of Vol. V. Ooo ftone stone like porphyry, but not so hard as marble. This

foft porphyry is composed like the hard.

With respect to what the curious call green porphyry, I rather suppose it to be a granate than a
porphyry; it is not composed of spots like the red
porphyry, and its substance appears to me to be
similar to that of a common granate. In Tuscany,
in the stone with which the antient walls of Volatera
were built, there is a great quantity of shell sish, and
this wall was built 2500 years ago. Most antique
marble, porphyry, and other stones of the most ancient monuments contain therefore shells, and other
wrecks of marine productions, as well as the marble
we at present take from the quarry. Therefore, it
cannot be doubted, independent even of the sacred
testimony of holy writ, that before the deluge the
earth was composed of the same matter as it is at

prefent.

From all what has been faid, we may be affured, that petrified shell fish are found in Europe, Asia, and Africa, in every place where chance has led observers of nature: it is also found in America, Brafil, Tucuman, the Magellanic land, and in fuch a great quantity in the Antilles, that below the cultivable land, the bottom which the inhabitants call la Chaux, is nothing else than a composition of shells, madrepores, aftroites, and other productions of the fea. These observations, which are certain, would have made me think that there are shells, and other petrified marine productions in the greatest part of the continent of America, and especially in the mountains as Woodward afferts; nevertheless M. Condamine, who was feveral years at Peru, has affured me that he had not feen any in the Cordilliers; that he had fought for them in vain, and that he did not imagine there was any : and this exception would be fingular, and the confequences that might be drawn from it, would be still more so; but I

own, that in spite of the testimony of this cele last naturalist, I shall have doubts, and am very much inclined to suppose, that in the mountain of Peru as well as elfewhere, there are shells, and other marine petrifications, but that he did not perceive them. It is well known, that in matter of testimonies, two positive witnesses who affert to have seen a thing, is fufficient to make a complete proof; whereas a thousand and ten thousand negative witnesses, and who only affert not to have feen a thing, can only raise a flight doubt thereon: It is for this reason, and from the strength of analogy which constrains me, that I perfift in thinking that shells are to be found on the mountains of Peru, as they are found elsewhere, especially if we search for them on the rife of the mountain, and not at the fummit,

The highest mountains are generally composed at top of rock, stone, granate, and other vitrifiable matters, which contain only few or no shells. All these matters are formed in the beds of the sand of the fea, which covered the tops of thefe mountains. When the fea left them bare, the fand fell into the plains, where they were carried by the rain, so that there remained only rocks on the tops of mountains which were formed in the interior parts of these beds of fand. At two, three, or four hundred fathom lower than the top of these mountains, are often found Imatters quite different from those of the summit: that is to fay, stone, marble, and other calcinable matters, which are disposed in parallel strata, and contain shells and other marine productions. Therefore it is not furprizing that M. de la Condamine did not find any shells on these mountains, especially if he fought for them in the higher parts of those mountains which are composed of rock, gres; or vitrifiable fand; but below those beds of fand, and those rocks which form the fummit, there must be some in the Cordilliers, as in all other mountains,

rizontal strata of stone, marble, earth, &c. where shells are to be met with; for in every country where observations have been made, they have al-

ways been met with in these strata.

But let us for a moment suppose this circumstance to be true, and that in fact there is no marine productions in the mountains of Peru, all that may be concluded from it will no ways affect our theory; and it might possibly have happened, that there is part of the globe which never has been under water, especially such parts as the Cordilliers are; but in this case, there might be some beautiful obfervations made on those mountains, for they would not be composed of parallel strata as the rest are: the matter also would be very different from those we are acquainted with; there would not be perpendicular cracks, the composition of the rocks and stones would not at all resemble the composition of the rocks and stones of other countries, and at last in these mountains we should find the antient structure of the earth, fuch as it originally was before it was changed and altered by the motion of the waters: we should see the first state of the globe in these climates, the old matters of which it was composed, the form, the bond, and the natural arrangement of the earth, &c. but this is too much to expect, and on too flight foundations; and I think we ought to confine ourselves to suppose that shells are to be found there as well as elfewhere.

With respect to the manner in which these shells are disposed and placed in the strata of earth or fand, Woodward writes as follows: "All shell sish which are met with in an infinity of strata of earth, and banks of rocks, on the highest mountains, and in the deepest quarries and mines, in slints, &c. &c. in masses of sulphur, marcasites, and other metallic and mineral matters, are silled of the matter itself, which form the banks, or strata, or the masses which

which include them, and never any heterogeneous matter, page 206, &c. The specific weight or different kinds of sand differ but very little, being generally with respect to water as $2\frac{4}{9}$ or $2\frac{9}{16}$ to 1, and the muscle shells, which are nearly of the same weight, are generally formed therein in great number, whereas it is very rare to meet with oyster shells, whose specific weight is but as $2\frac{4}{3}$ to 1; of sea cockles, whose weight is but as 2 or $2\frac{4}{8}$ to one, or other forts of lighter shells; but on the contrary in chalk, which is lighter than stone, being to water but as $2\frac{4}{10}$ to 1, we find only cockle, and other kinds of lighter shells.

It must be remarked, that what Woodward says in this place, must not be looked upon as a general rule, for we find lighter and heavier shells in the fame matters; for example, petoncles, oysters, &c. in the same stones and earth, and even in the Royal cabinet, may be feen a petrfiied muscle in cornaline, &c. therefore the specific weight of the shells has not influenced so much as Woodward supposes, their position in the earth, and the true reason why such light shells are found more abundantly in chalk, is, that chalk is only the ruinated part of shells, and that those of the oursin being lighter, less thick, and more friable than the rest; they have been eafily reduced into powder and chalk, fo that strata of chalk are only met with in the places where formerly a great abundance of these light shells at the bottom of the sea, where wafte have formed chalk, in which we find those which having refisted the frictions, &c. are preferved entire, or at least in parts large enough not to be discovered.

But this is treated more fully in our discourse on minerals, let us then here content ourselves with faying, that a modification must be given to Woodward's expressions; he seems to say that shells are to be found in flints, cornaline, in mines, maffes of fulphur, as often, and in as great a number as in other matters: whereas, the truth is, that they are very rare in all vitrifiable or purely inflammable matters: and on the contrary, are in prodigous abundance in chalk, marl, marble, and stone, infomuch that we do not here absolutely pretend to fay, that the lightest shells are in the lightest matters, and vice verfa, but only that in general they are oftener found so than otherwise. In fact, they are all equally filled with the fubstance which furrounds them, as well those found in horizontal strata as those found in a smaller number, in matters which fill up the perpendicular cracks, because, in fact, both have been equally formed by the waters, although at different times and in different manners. The horizontal strata of stone, marble, &c. having being formed by the great motion of the waves, and flints, and all matters which are in the perpendicular cracks, having been produced by the particular motion of a small quantity of water, loaded with lapadific metallic juices, &c. and in both cases these matters were reduced into a fine and impalpable powder, which has filled the shells so fully and absolutely, as not to have left the least vacuum.

There is therefore in stone, marble, &c. a great multitude of shells which are whole, beautiful and so little changed, that they may be easily compared with the shells preserved in cabinets, or found on the sea shores. They are precisely of the same figure and size; of the same substance, and their tissue the same; the particular matter which composes them

is the fame, disposed and arranged in the same manners, the direction of their fibres and spiral lines are the same, the composition of the small lama. formed by the fibres is the same in the one as the other; we see in the same part, vestiges or insertions of the tendons, by means of which the animal was fastened and joined to its shell; we see the same tubercles, stria and pipes; in short the whole is alike, whether within or without the shell, in its cavity or on its convexity, in its substance or on its superficies: in other respects these fossil shell fish are fubject to the same common accidents as those of the fea; for example, the least are adherent to the large; they have vermicular conduits, pearls are found therein, and other fimilar matters which have been produced by the animal when it inhabited its shell; their specific gravity is exactly the same as that of their kind found actually in the fea, and by chemistry, we meet with the same matters therein; in a word, they exactly resemble those of the sea.

END of the FIFTH VOLUME.



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